Thinking is a high-level skill in which symbols—words, numbers, shapes, colours, tones—supplement and may take the place of, bodily movements. In recent years great advances have been made in the experimental study of human skilled behaviour, and these have suggested a fresh approach to problems of thinking. Sir Frederic, who has himself initiated so much original experimental work, introduces a number of simple experiments which can all be repeated by anybody who is interested. They show that the thinker is all the time trying to fill up gaps in information that is available to him in such a manner that there is a good prospect that all other thinkers, given the same incomplete information, will agree with him. How, and what are the conditions, under which he does this, are considered and illustrated (a) for formal thinking; (b) for the thinking of the experimental scientist; (c) for 'everyday' thinking, and (d) for the thinking of the artist.

A great many of the processes used in thinking have been developed at a level of bodily skill, and long before thinking proper becomes possible at all. At the same time, it becomes clear, as the investigation proceeds, that thinking processes have important characteristics and rules peculiar to themselves. These also vary according to the fields of information in which the thinker operates. There is no doubt that Sir Frederic's experimental study is a work of first importance.

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An Experimental and Social Study

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THINKING

An Experimental and Social Study

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PREFACE

As I have indicated in Chapter 8 of this book, it was my purpose, as far back as in 1932, to develop an experimental study of the large variety of processes which people call "thinking." A beginning was made then, but it had not proceeded very far before the second Great War came, and for sufficient reasons my interests, and those of most of my colleagues, were diverted to experiments and reflexions about bodily skill, its basic characters, and the conditions of its acquisition and practice. All the time the possibility of developing experiments upon thinking which would differ from the traditional approaches remained more or less active at the back of my mind.

If I had attempted to write this book in the 1930's it must have included some detailed and critical account of earlier psychological work. Fortunately Professor George Humphrey published his splendid study of the classical experimental psychology of thinking in 1951,[1] and there is now no need for me to try to repeat what he has already done with complete authority.

Recently a great revival of interest in problems of thinking has occurred and much has been published. But here also there is available plenty of readily accessible and reliable information. Extensive references, and much discussion, for example, may be found in the brilliant and original book by Professor Jerome Bruner and others entitled *A Study of Thinking*.[2]

It will be noticed that the present volume contains comparatively few specific references to the work of other psychologists, earlier or contemporary, about thinking. In writing it, in fact, I was not concerned to produce anything

[1] *Thinking: An Introduction to its Experimental Psychology*. London: Methuen 1951.

[2] New York: John Wiley & Sons, London: Chapman & Hall 1956.

like a systematic treatise. I had three principal aims in mind. First to try to put thinking into its place as a natural development from earlier established forms of bodily skilled behaviour. Secondly, on the basis of this approach, to design some more or less novel experiments of a predominantly objective type, and such that, I hoped, anybody with sufficient interest could carry further for himself. Thirdly, to illustrate a few of these experiments, and to embark upon some discussion of their results. The book is in no sense final. If it should succeed in starting more and better explorations in the spacious field of its study, it will have served its purpose.

No author, writing a book of this kind, can possibly acknowledge all his debts to other people and to varied sources of information. Many of them he may not even himself know. I owe probably most of all to generations of Cambridge students who have taken part in discussion classes, and to colleagues in the University of Cambridge Psychological Laboratory, and the Medical Research Council Unit for Research in Applied Psychology established at Cambridge since 1944. To the Medical Research Council itself and to St. John's College in the University of Cambridge, I am deeply indebted for long-continued support and encouragement. Dr. E. F. Gale, F.R.S., and Dr. J. S. Mitchell, F.R.S., both read and criticised the chapters on Experimental Thinking, though naturally any views that these chapters express are my own responsibility. I particularly wish to express my thanks to Mrs. V. Simmonds for preparing the various drawings that have been included in the text, and to Miss Pauline Dyson for much valuable editorial work.

F. C. BARTLETT

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CONTENTS

CHAPTER ONE

Thinking as a Form of Skill

1. THE BACKGROUND

One thing, at least, about thinking is absolutely certain. All the various ways in which it is practised by human beings have become possible only as a result of prolonged processes of biological development. For many centuries thinking was regarded as a sort of gift, specially presented to man, and the most important and decisive of all the differences which distinguish man from the other animals. No serious and informed person can any longer accept this as the correct view.

It follows that if we want to find out something definite about the nature and conditions of thinking processes, our best chance is to make use of clues that may be available from the study of simpler but related behaviour. In fact, if we do not do this, it seems as if the only alternative is to construct some general theory or other, and then to use observation and experiment to justify this theory. The result is bound to appear both tautologous and to some degree arbitrary; and in any case it is not what the scientist wants.

2. THINKING AS HIGH-LEVEL SKILL

It seems reasonable to try to begin by treating thinking provisionally as a complex and high-level kind of skill. Thinking has its acknowledged experts, like every other known form of skill, and in both cases much of the expertness, though never, perhaps, all of it, has to be acquired by well-informed practice.

Every kind of bodily skill is based upon evidence picked up directly or indirectly from the environment, and used for the attempted achievement of whatever issue may be required at the time of the performance. Every kind of thinking also claims that it is based upon information or evidence which, again,

must be picked up directly or indirectly from the environment and which is used in an attempt to satisfy some requirement of the occasion upon which the thinking takes place.

It would be easy enough to go on pointing out a number of other likenesses, of a similar general character, between bodily skill and thinking. But it is extremely important to realize that the case for beginning a study of the thought processes by using as clues what is already known and established about the measurement and nature of bodily skill, does not, in any important sense, rest upon these.

Everybody who has ever attempted to study thinking has had to admit that it is a very complicated affair, with many different modes, with varying objectives that may seem to have little or nothing in common one with another, and with a wealth of circumstances which can influence its direction and its efficiency. Particularly if an experimental approach is tried there is so much to observe that unless we have some clues to give an initial direction to our observations we are almost certain to get bogged down in an unmanageable mass of detail. The case for building upon what is known about bodily skills is essentially methodological and pragmatic. We must have some definite points of departure, but points of departure do not normally, and certainly need not, themselves dictate either the direction of travel or the end of the journey. In this instance we are in no way committed to an attempt to establish an elaborate analogy between bodily skills and thinking processes. In fact we know that whenever any biological process is built up upon functions simpler than itself it is almost sure to acquire its own peculiar characteristics and to have its own special conditions. We can, and indeed we must, use what has been established about the simpler functions, to provide us with a line of approach, but we are not to suppose that exploration will reveal nothing that is radically new.

The last fifteen years or so have produced a marked advance in our knowledge of how to set about the measurement of bodily skills, and of their leading, critical features. It seems not unreasonable to hope that a brief review of the present position may indicate workable methods and definable problems for an experimental approach to thinking.

3. PROPERTIES OF SKILLED PERFORMANCE

Nobody normally uses the term "skill" of the very simplest kind of behaviour, where what can without violence be treated as a single stimulus gives rise to what can be treated, equally without violence, as a single, and isolated, response. We do not consider experiments to determine an absolute threshold—for example, the smallest amount of light, or sound, that can be seen, or heard—as experiments on the measurement of skill. In the same way a determination of reaction-time is not, in itself, a measure of skill; or, in fact, is any quantity which expresses the character of a single item in, say, a repetitive task when that item is taken out of its setting and produced under exactly determined and constant conditions. These are all highly abstract items, or constituents of behaviour, made possible and measurable only by carefully controlled experiment, and although their study is enormously important for many reasons it can contribute nothing of real moment to an understanding of skill. It is perfectly true that thresholds, reaction times, items of behaviour taken out of their setting, can all be shown to be capable of modification as a result of repetition. And this can be taken to indicate that they are, in fact, more dependent upon what precedes and follows them in the experiments than is usually recognized. But they are not ordinarily studied from this point of view, and they are not very good cases to take, partly because the range of possible modification is small, and partly because the experimental situations in which they can be investigated are already so simplified that it is difficult to find out anything definite about the modifications, except that they take place and what they are.

Even these very simple cases obviously demand a working combination of receptor and effector functions; but we begin to use the term "skill" only when a good many receptor and effector functions are interlinked and related within an order of significant succession which possesses an inherent character of direction and moves towards an issue regarded as its natural terminus.

This essential requirement of any performance that can be called skilled becomes much more plain if we look at a few actual instances. The player in a quick ball game; the operator,

engaged at his work bench, directing his machine, and using his tools; the surgeon conducting an operation; the physician arriving at a clinical decision—in all these instances, and in innumerable other ones that could just as well be used, there is the continuing flow from signals occurring outside the performer and interpreted by him to actions carried out; then on to further signals and more action, up to the culminating point of the achievement of the task, or of whatever part of the task is the immediate objective. From beginning to end the signals and their related actions form a series, not simply a succession. Skilled performance must all the time submit to receptor control, and be initiated and directed by the signals which the performer must pick up from his environment, in combination with the other signals, internal to his own body, which tell him something about his own movements as he makes them. These are the main reasons why all forms of skill, expertly carried out, possess an outstanding character of rapid adaptation. For the items in the series have, within wide limits, a fluid order of occurrence and varying qualities. So what is called the same operation is done now in one way and now in another, but each way is, as we say, "fitted to the occasion."

4. EXPERIMENTS ABOUT BODILY SKILLS

It is of interest that none of the increased understanding of the psychology of skill which has been won was started from a formal analysis of laboratory situations. The initial impetus came from direct and, as far as possible, unprejudiced observation of practices and activities that everybody would agree to call skilled. It is equally true and equally interesting that once the working ideas had been suggested by direct observation, further definite progress was achieved only as it became possible to put these working ideas into operations that could be built for the laboratory and tested under reasonably well-controlled conditions.

(*a*) "*Timing*"

The situation in which signals and responses to signals can be set into an order of significant sequence is not a difficult one

to study experimentally.[1] When this is done it becomes readily demonstrable that by far the most important characteristic of expert bodily skill is "timing," and also that timing has little or nothing to do with the absolute speed at which any component response in the skill sequence is performed. Efficiency depends, more than upon anything else, upon the regulation of the flow from component to component in such a way that nowhere in the whole series is there any appearance of hurry, and nowhere unnecessarily prolonged delay. R. Conrad, particularly, in a number of most elegant and penetrating experimental studies, has shown that bodily skills inevitably have a temporal structure and also that this can become defined and smoothed through exercise, so that the initially long intervals between components are shortened, and the short intervals become lengthened. The whole performance then takes on that character which is perhaps more often than any other used by the critic as the leading criterion of expertness in any form of bodily skill: that the operator has "all the time in the world to do what he wants."

If we look into this more closely we find that it means that no single component in skilled behaviour is a function merely of that signal which immediately starts the response going. Within limits that can be experimentally determined and measured, surrounding signals and responses in both directions are contributing their shares. In the actual performance of most, and perhaps all, forms of bodily skill the temporal limits of this kind are rather narrow. It is only the near past and the near future that count. Moreover, it seems as if it is the near future—"anticipation of what is coming next" if a psychological description is required—that plays the principal part in producing that objective smoothness of performance which is the hall-mark of a high quality of skill.

Another way of putting all this, and perhaps the most interesting of all if we are looking for clues about how to

[1] Excellent illustrations of fairly simple and readily constructed arrangements that can be used for this sort of experiment may be found in Conrad, R.: *Speed and Load Stress in a Sensori Motor Skill*, Brit. J. Industr. Med., 1951, 8, pp. 1–7 *Timing*, M.R.C. Appl. Res. Unit, 1953, Rep. No. 188; Leonard, J. A.: *Advance Information in Sensori Motor Skills*, Quart. J. Exp. Psychol., 1953, 5, pp. 141–9; Singleton, W. T.: *The Change of Movement Timing with Age*, Brit. J. Psychol. 1954, 3, pp. 166–72. Many others could be given.

design experiments on thinking, is to say that the immediate evidence, at any moment of skill behaviour, is incomplete. It leaves gaps on both sides. On the side of the near past the gap has already been filled, but to make use of it may require a mechanism of short-term storage, or, to use the psychological expression, of immediate memory. On the side of the near future the gap still remains partially unfilled; the operator must be accepting evidence which has not yet reached the stage for action, at the same moment at which he is already acting upon other evidence.[1]

When any bodily movement is performed as a component in a skill sequence, three time measures relating to that movement are always possible. These are: the reaction-time, the time which elapses between the immediate signal and the beginning of the movement; the movement time itself, and the interval between this movement and the next succeeding movement. It is possible, and indeed usual, for all of these to vary while any measure of whatever it is that the movement achieves remains constant. Hence it is that measures of the overall achievement in the case of skill behaviour very rarely throw any light upon the processes involved in the achievement itself.

(*b*) *Stationary Phases of Bodily Skill Performance*

Of the three measures far the most unstable, and also the one that contributes most to an understanding of the skill processes, is the third. A considerable number of investigators, studying manipulative skill from many different points of view, have agreed that the variable elements in most psycho-motor skills are not the travel components but the "halts" or intervals from one direction of travel to another.

It is, for example, a leading feature of many bodily skills that when a moving object reaches a certain position relative to an operator, the latter must take appropriate steps to despatch it, or transport it, with considerable accuracy, to another position which may either be decided by him (as in most ball

[1] I have attempted to work out the variety of functions of "Anticipation in Human Performance" in an article with this title published in *Essays Presented to Professor David Katz*. See also N. H. & J. F. Mackworth: *Remembering Advance Cues during Searching*, M.R.C. Appl. Res. Unit, 1957, Rep. No. 258.

games) or assigned to him (as in industrial assembly operations). In all such cases the operator's performance consists of a series of bodily movements with interspersed halts. For instance, in the simple case of picking up an object and transporting it to another, assigned, position, the hand stretches out, hovers, picks up, transports, hovers again and finally sets the object down. The interesting thing is that the hovering, or halting, almost always occurs just when those points are reached at which it becomes necessary to pay attention to environmental characters such as the exact position, the size, shape, probable weight of the object, and in many cases the disposition in space of a number of objects relative one to another. That is to say, the stationary components of what may seem to be continuous skilled movement make their appearance when something that is novel in so far as the movement is concerned, has to be *perceived*, when, therefore, central neurological processes of a more complex order than those which bring about the movement itself must come into play. It is these hoverings or halts which are the most important regulators of the "timing" which characterizes smooth and efficient skill performance.[1] More readily than any of the other components they can be prolonged or shortened. If they are unduly prolonged subsequent movement must be hurried; if they are unduly shortened, subsequent movement must be slowed up. Both are difficult and lead to awkwardness and errors.

From our present point of view this means no more than that if we are going to use what is already established about bodily skills to guide an experimental approach to thinking, it ought to be worth while looking to see whether, and when, how and with what effects, halts occur in the thinking activities.

(*c*) *Point of No Return*

Another character which belongs to all bodily skills is perhaps most vividly described in the airman's phrase "the point [more accurately 'the region'] of no return." Skilled performance

[1] For some of the experimental evidence see Seymour, W. D.: *Manual Skills and Industrial Productivity*, J. Inst. Prod. Engrs., 1954; Singleton, W. T.: op. cit. and "Deterioration of Performance on a Perceptuo-Motor Task" in *Symposium on Fatigue* (London: H. K. Lewis, 1953, pp. 163–72). Further references are given in these papers.

reaches a stage beyond which the further input of signals cannot produce a result because it fails to be noticed, or does not produce a result because it is ignored. Alternatively, the new signals lead to a belated attempt to modify action and error follows. The experimental evidence is conclusive. It has been shown by many investigators and for many different types of skilled response, that once an action is launched and carried to a certain stage, any additional signals will either be ignored or will lead to an attempt to modify projected movements under conditions in which accurate timing is impossible.

Apart from exactly controlled experimental situations, there are many illustrations of the importance of the "region of no return" which anybody can easily observe as he watches, say, a quick ball game. In cricket, for instance, in the battle of wits between the bowler and the batsman, the former is always trying to deliver a ball which does something out of the ordinary after the latter is already well launched upon his intended stroke. The expert batsman, on the other hand, either moves to meet the ball, so that it has no chance to do something unexpected at a late stage of its flight, or he is able so to delay the actual launching of action that he has no need, at the last moment, to change his stroke.

It has frequently been pointed out that thinking, especially of the more logical and rigorous kind, seems to possess to an outstanding degree a property of inevitability. When it has reached a certain stage in a given direction it appears to become obstinate, so to speak, about additional evidence, or even impervious to such evidence, as if it also has now reached a "region of no return." If we could devise experiments to show whether this occurs or not, and if it does under what conditions and with what results, it seems reasonable to hope that we might get some new light on the basic characters of error in thinking.

(*d*) *Direction*

Again and again in the course of this discussion phrases have recurred such as "significant sequence"; "moving towards an issue"; "satisfying the requirements of evidence." It is, in fact,

impossible to continue very far in any realistic study of bodily skill without introducing the function of a guiding and controlling *direction*. So far as skilled movements are concerned, this direction has to be regarded as a property which belongs, or comes to belong, objectively to the movements themselves as they are performed. Sometimes, though not, perhaps, in the majority of cases, the directional property is also perceived or acknowledged by the operator while he is at work. But it still remains very much of a puzzle how, and by what human mechanisms, directionality can be perceived and come to be not only a property of the movements themselves but also of what is known about them by the person who makes them when he makes them. Whatever the specific mechanisms may be, since they essentially have to do with the relating of movements in series it is certain that they are a central, neurological affair. If it is useful to look upon thinking as a high-level form of skill it is likely, therefore, that there must be at least some kinds of thinking in which the character of direction assumes a preponderating influence. Moreover, it also seems extremely likely that if we are dealing with a high-level skill, in such cases far more often than at a lower level the directional property, to use a phrase which has lost some of its former popularity, "rises into consciousness," so that it can be defined and formulated by the thinker himself at the time at which he is exercising his skill. At least we ought to look for evidence for or against this; and should the evidence be positive, come to grips with the fascinating but very difficult problem of how directionality can be perceived or in some other way appreciated.

5. TRANSITION TO THE STUDY OF THINKING

As a result of this preliminary discussion we have now a possible method of approach to the problems of thinking which specifically relates them to the background of achievement out of which they have most certainly grown. We can build upon those experiments which have been most fruitful in the study of this background, and we have a number of clues from these experiments which can be used to direct our search. Obviously we must not yet decide that there are no characters or func-

tions of our supposed high-level skill which fail to appear at lower level. We shall almost certainly find both more and less than we have at this stage set out to look for. And even when the characters and functions are descriptively the same they may have their own high-level parts to play.

These are the points that are important at the moment. First, that we should be content to regard thinking as an extension of evidence, in line with the evidence and in such a manner as to fill up gaps in the evidence. While we need not dogmatically suppose that the gaps are inevitably filled by a series of interconnected and articulated steps, it is simpler and, in fact, experimentally necessary, to begin with instances in which this does happen. We can then try to find whether "timing" has that function and that importance in thinking that it demonstrably has in bodily skill. We can look for "halts" between steps, or impose halts between steps and find out what are their effects. We can see whether thinking, like lower forms of skill, has its "region of no return." We may perhaps be able to discover the sorts of instances in which the appreciation of directionality becomes all-important, and we may even be able to reach some accredited views about how it may be achieved. All this we ought to be able to do in at least a mainly objective manner, and without that almost exclusive resort to what the thinker can say about his own performance which has been a mark of far too much of the earlier work in this field.

Unquestionably it could be said that even so broad a programme of approach as this may to some degree pre-judge the issues. Little good purpose would be served by any attempt to consider such a view fully at the present stage. If it is a limitation, however, it is a limitation inherent in the very use of experiment. Everybody now recognizes that fruitful experiments can be designed only when problems are to a degree defined and made precise and that this must involve some selection both of the material which is to be observed, measured, and analysed, and of what are to be treated, in a preliminary way, as the key constituents of this material. We have at least now got a few guiding methods and problems in mind and can proceed to apply and study them.

CHAPTER TWO

Thinking Within Closed Systems—1

INTERPOLATION

1. THE USE OF EVIDENCE TO SUPPLEMENT EVIDENCE

I shall now proceed to describe and discuss some of the experiments which I designed and carried out in order to test and develop the general approach already considered. There is no suggestion that other approaches could not, or should not, be attempted. Biological activities are often expressed in different ways and through a variety of mechanisms, and thinking forms no exception. Any sound or suggestive experimental approach must be prepared to consider different possible ways into the problems that are involved. Also it may well be that there are some psychological problems about thinking which cannot be avoided, but are hardly amenable to experimental study, at any rate at the present time.

We may find, as we go along, that the experimental methods that are being considered themselves raise some difficulties which we cannot resolve, at present, in any other way than by "taking sides." I shall assume that we need not be daunted by this, but that there is an obligation to be frank about it. All that is being claimed is that these experiments, growing from an attempt to treat thinking as a relatively high-level form of skill, are able to define and sharpen a number of important questions, and to go at least some way towards establishing answers to these questions.

All the experiments employed possess a few leading characters. The general pattern of these will already be familiar to everybody from daily experience. We meet somebody, for example, and then do not see him again for some time. When we do next meet him we find that his appearance, or his manner,

has changed. At once we begin to try to think what sort of things can have happened during the interval to account for the change. Or we borrow a book, begin to read it, and then find that there is a missing page. We use the information available where the gap begins, and that available where it ends, to fill up the gap. A man is tracking a target, it may be a submarine, or an aeroplane. He picks it up, loses it, picks it up again. He must use what he knew about the target when he lost it, and what he now knows when he has found it again, to think, or to "build up," what probably happened while the target was lost.

All of the experiments I shall consider are cases, some of them simple, some of them complicated, of this kind of thing. Information is given, and then more information, and between there is a gap. The gap is required to be filled in accordance with whatever evidence is available from the incomplete information supplied. In other instances information is presented, carried on, or developed, through a few steps, and then left in an obviously incomplete stage. The evidence must be further developed until it reaches what is to be treated as a terminal point. Or it may be that all the items of a situation are given, but something still remains to be done with them. They have, for example, to be brought into specific relation, through a series of steps, in such a manner that they satisfy a requirement laid down when the items were given.

These are the three kinds of gap-filling processes, and all thinking appears to illustrate one or more of them. In the first the gap is filled by interpolation, in the second by extrapolation, and the third requires that the evidence given should be looked at from a special, and often an unusual point of view, and that it should be recomposed and reinterpreted to achieve a desired issue.

In all these typical cases of more or less controlled gap filling, it is experimentally necessary that the initially empty intervals should be filled by series of steps which are expressed, or articulated, and not simply left to the imagination either of the experimenter or of the operator. No doubt, it could be argued that there is something artificial about this demand that gaps in evidence should be filled up by a series of interrelated

and expressed steps. When information that calls for completion, or extension, occurs in ordinary life, it may be that in many instances, perhaps even in most, the completion or extension is achieved, so to speak, "all in one go." It may even be that if a series of steps are set out which lead to an alleged terminal point, it is more common for the steps to be reached through the terminal point than for the terminal point to be reached through the steps. Whether, or in what senses, the demand for articulated and expressed steps renders the experiments "artificial" will have to be considered more carefully and critically later on. For the moment I shall simply admit that it is a demand inherent in the methods which are going to be illustrated and discussed, and that it cannot be avoided.

2. CLOSED SYSTEMS

Thinking within closed systems is neither the commonest nor is it, in any sense of the term, the "simplest" form of thinking. Nevertheless, it is most convenient to begin any discussion of the results of an experimental approach with this, because it is the form of thinking which is most readily amenable to controlled investigation. For our present purpose a closed system is defined as one possessing a limited number of units, or items, or members, and those properties of the members which are to be used are known to begin with and do not change as the thinking proceeds. The units, items, or members, however, may be arranged into a variety of orders or relations, and although the properties of the individual constituents remain stable from first to last, other properties of orders, or sets of constituents may appear to develop, in a psychological sense, as the thinking goes on. One way of putting this, perhaps, is to say that all the items to be used are theoretically definable before they come to be used in any particular instance. We shall find that this is not the case in a great amount of scientific thinking, in popular and socialized forms of thinking, in artistic thinking, or in the kind of thinking whose only expression seems to be a highly skilled use of the muscles of the body.

The best illustrations of closed systems are numerical, geometrical and formal logical. It is, however, possible to put any sort of constituents into the closed system form provided that whenever the occasion arises to use these constituents each of them is already unambiguously defined and the definition is thereafter maintained without change. Perhaps it may also seem that there can be no thinking within any kind of system, closed or open, unless the constituent items, besides being defined, are treated in accordance with "rules" of some kind. But whether this is the case, and in what sense, are best left for discussion until some of the experimental evidence is available.

3. A SIMPLE NUMERICAL SYSTEM

The first instance I will consider is the simple one in which the information given is

1 17,

and the experimenter says "take 1 as the first and 17 as the last number, and fill up the gap between them in any way that seems to be indicated."

There will always be a few people who will treat any kind of a presented gap in information as an opportunity to exercise a "flight of fancy." That is, they will fill up the empty interval with drawings or words, or phrases, or any sort of preferred symbols, without reference to the character of the items of information that are given. For the present I am going to ignore these people and consider only that very large majority who accept the evidence given in this instance as an indication that they have to work in a number series.

(*a*) *Limit of Interpolation Range*

Obviously there are a large variety of ways of moving through a number series from 1 to 17. But, as we shall find to be the case always, the variety of ways in which a gap will be filled is very far short of the variety of ways in which theoretically it might be filled. In this instance, at least if an operator population is taken at random from Great Britain or

America, there are only four ways that need be seriously considered. They are, in the order of their frequency:

(*a*) 3, 5, 7, ... 17;
(*b*) 2, 3, 4, ... 17;
(*c*) 9 . . . . 17;
(*d*) a number of other ways, each of which occurs rarely, so that it is possible to treat them as all falling into a single group of "individual responses."

Of these, so far as present results go, to the nearest whole number, for every instance of (*d*) there are two of (*c*), 5 of (*b*), and 6 of (*a*). Neither these nor any other frequency figures that may be given later are to be taken as necessarily final. But it seems very unlikely that a larger survey, so long as it dealt with a similarly educated population, would show any radical or substantial differences of distribution, and it is quite certain that the same general principles would be illustrated.

(*b*) *Determining the Number and Order of Steps*

Whenever incomplete evidence is completed by the interpolation of additional steps, if there is a sense in which the *number* of steps has to be considered as determined, it is always possible also to raise the question of the determination of *order* among the steps. In so simple and elementary an instance as the one which we are now considering the differences between step number and step order determination are neither very striking nor very important. But they are found even at this level and as we move to instances offering greater ranges of probable steps, and, especially, less completely predetermined terminal points, we shall find that the ways in which the number and the order of steps are controlled become more and more interesting and significant.

In the transition from 1 to 17 the *number* of steps is, in the vast majority of cases, settled as soon as ever the first step has been taken. One of the questions that has always to be asked if this kind of experimental approach to thinking is adopted is "How much (or what sort of) information must be given if the gaps presented are to be filled up in a completely, or near completely, uniform manner?" In this case the results of the

trials show that if one more item of evidence is added the *number* of steps from 1 to 17 is, to all intents and purposes, completely settled. Indeed it seems as if, when a gap in evidence has to be filled by interpolation, and each successive step differs from its preceding step in one dimension only, the minimum information necessary for uniform filling consists of three items: the beginning item, the terminal item, and the first step from the one to the other, or, in some cases, the last step of all. There will always be a few people who will need more than one step, but when there is merely a one-dimensional directional change they are very few.

This is not the full story, however, about number of steps. Even in so elementary a case as this one there are always a few people who, so to speak, "break the rule of the steps" as they proceed. For example, they will go

1, 3, 5, 9, 13, 17

or 1, 3, 7, 11, 13, 15, 17

or 1, 4, 7, 10, 13, 15, 17.

There are, of course, many variants of this kind of thing, the outstanding characters of which appear to be

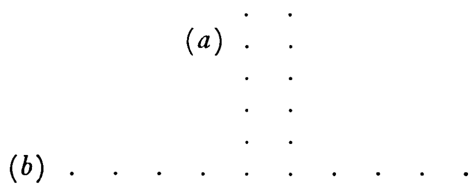
1. the replacement of one rule of sequence by another which, without or with subsequent change, will reach the terminal point by fewer steps;
2. a delayed recognition that a rule of sequence adopted will, if it is continued, not reach the assigned terminal point at all, and a consequent replacement of the rule first adopted by another one.

A rule in sequence may be replaced or broken and the person who does it may fail entirely to notice what he has done. If he does notice it he is always a bit unhappy, as if moving from one rule, or regularity, to another in a closed system series is in some way a mistake. Very often he will "correct" his attempt, and if he does the number of steps required is always treated as less important than maintaining an entirely regular sequence.

However, the topics of errors and of the correction of errors in thinking are of such interest that their discussion is better postponed until a greater variety of illustrations is available.

Settling the order of steps is never the same thing as settling their number, and even in the simpler cases more evidence, or information, must be given if a uniformity of order is to be achieved. Thus 1, 3 17 practically determines the number of steps that will be taken to complete the series; but they may be taken in any order. To give an additional item in this way; 1, 3 13, 17, will, it is true, set a majority of people building up the gap bridgewise from either end; but the agreement as regards order still remains less than agreement as to number.

A type of interpolation situation which illustrates clearly how number of steps can be completely, or almost completely, determined while their order is left fluid is the following:



Arrangement (*a*) passes into arrangement (*b*) through a number of steps, or moves. At each step one dot only is moved, along a straight line in a single direction. The observer has to indicate the moves by which he would change (*a*) into (*b*), show their direction by lines with arrow heads, and number the order of moves. The overwhelming majority of people achieve the required rearrangement in eight moves; a few take ten moves, but nobody more. The order of moves, however, is about as varied as it possibly can be. If a large number of attempts are compared it becomes clear that predictions about order of steps made at the outset of any such set of operations are extremely precarious, and that even after several steps have been taken, the range of variation in subsequent steps remains considerable.

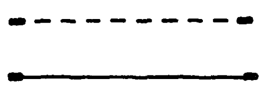
In fact there are many sorts of sequences such that, while information given in terms of the constituent items can absolutely determine the number of steps, there is no way by which information in terms of items can absolutely determine order of steps. There are, of course, empirical limits of order

variation, but they are usually wide and their probability values have to be experimentally established. Another way of putting the same thing is to say that in this kind of sequence there is nothing inherent in any step which links it unequivocally to some other particular step. The only way to ensure uniformity of order is to issue a command, or an instruction, and get it accepted.

4. NUMBER AND ORDER OF STEPS IN SIMPLE FORM SYSTEMS

It is not difficult to find closed systems within which both the number and the order of steps used to fill up a gap are, within quite large limits, determinable only by command. Any gap in information which has, say, a single short line, or combination of lines, or some simple closed figure at one extreme and precisely the same at the other extreme, illustrates this. In all such instances the nature of the steps used to fill up the gap remains remarkably uniform for observers belonging to the same, or nearly the same, cultural group. As always, in their descriptive characters, the steps used vary much less than they theoretically might do, and the probability of any given type of step can be experimentally determined.

For example, if a gap is presented as follows, to be filled up with regard to the first and last steps given; ▬ ▬ ; and if people are taken at random from this country, except for infrequent fantastic fillings, only two completions need be considered. These are



and ,

the former, according to my results, being just about twice as likely as the latter.

If the completion is by a single unbroken line the question of number and order of steps does not arise. In the other case it does, but there is nothing *inherent* in any step to settle either how many steps there should be or which step should come next. It will readily be realized that the number and variety of

gaps of this sort that could be presented for filling is endless. When any one is completed the result can be seen as a system possessing some kind of structure, usually a highly repetitive one, but literally no internal property of direction.

5. INTERPOLATION WITHIN A SIMPLE VERBAL SYSTEM

In the general experimental procedure adopted I gave my observers several cases in succession of the number system type already illustrated but of increasing complexity. I then said: "We will switch to a word gap. Look at the terminal words and then fill up the gap in any way that you think to be indicated." The items and arrangement given were:

A, BY, HORRIBLE.

(*a*) *A Problem of Transfer*

Thinking has almost always been supposed to possess some immediate generalizing character, or capacity, and I was prepared to find that there would at once be transfer from the number series to the word series at least in a good many instances.

However, out of between two and three hundred attempts there were only two clear instances of what could be called immediate and complete transfer, in which the initial letter of each successive word, in the sequence selected to fill the gap, was the next in order in the alphabet, and each word in the sequence had one letter more than its predecessor.

There were three other instances in which an alphabetical sequence of initial letters was adopted, but without control of the number of letters in successive words; and two instances of words increasing by one letter in succession but without regard to initial letters.

In all the other cases, attempts were made to build up meaningful sentences beginning with A and finishing with HORRIBLE. Many of them were extremely ingenious, but they had nothing in common except that they were word sequences and they call for no further comment at the moment.[1]

[1] See also pp. 37–40.

(b) Uniformity of Interpolation Steps

It now seemed desirable to try to find out how many items of information, in terms of acceptable members of this word sequence, would ensure uniformity of procedure as regards unsupplied steps. I therefore said "I am going to give you another item of information" and showed

A, BY, COW, HORRIBLE.

All seven of the observers already referred to now produced word series in which each successive item had the next alphabetical initial, and also one more letter than its preceding item. There was also a small increase in the number who now adopted the regular step-wise double increment, and a somewhat more marked increase in the number of observers who produced series varying regularly in one, but not both, of the critical features. The great majority continued to try to make up a meaningful sentence. The addition of a third step of information, or evidence, made a big difference, so that there was now rather more uniformity than variety of procedure. A fourth step brought nearly all observers into line.

It began to appear possible that the amount of evidence, in terms of items, or "chunks" of items, necessary to ensure a preponderance of uniformity in gap-filling may be in part a function of the number of directions, or dimensions of change which a system is simultaneously undergoing. Also it seemed likely that any added variation of direction or dimension would necessitate a more than proportional increase in items of information.

It may be unnecessary to point out that any word series in which words available for use are left to the individual observer, is bound to remain "open" to a large degree. For example, the two cases in which this particular series was completed "successfully" on smallest information given were:

(*a*) A, BY, CAR, DIVE, EAGER, FRIGHT, GASKETS, HORRIBLE.

and

(*b*) A, BY, CAN, DOOR, EVERY, FLOODS, GUNNERS, HORRIBLE.

(*a*) and (*b*) are uniform as regards sequence of initial letters, of relative lengths of words, and of total number of words in

their systems. But they differ completely as regards word items, except in the case of those items which were included in information given. In all such systems a large amount of freedom in the selection of items, other than those given for information, is consistent with a strict determination of rules of relation among the items. It seems as if this may have much to do with when and how thinking so works as to facilitate transfer. When and why do the items take charge, and when and why rules of procedure?

(c) *A Suggestion Concerning Intelligence*

It was at this stage of the investigation that I began to wonder whether there may be a direct relation between capacity to utilize minimal information (in terms of numbers of items) so as to build up sequential arrangements identical with those which very nearly every normal person will ultimately achieve, as information is increased, and high ranking intelligence. It certainly seemed to me that among the observers whom I knew well, or about whom I could get reliable information, the ones who achieved the most consistent results with the least amount of information were also those who were generally regarded as the most intelligent. To establish any such relationship could not, of course, be a main aim of these studies, but the possibility was an intriguing one and could be kept in mind.

6. THE MAIN POINTS SUMMARIZED

The general "pointers" which emerged from this limited experimental study of interpolation within closed systems may now be summarized. They are to be treated as nothing more than "pointers," and all held subject to further critical discussion.

1. The number of ways in which gaps within closed systems, whether of numbers, of forms, or of words, will be filled in any fairly homogeneous social group are far fewer than the number of ways in which they theoretically could be filled. Probable fillings cannot be calculated on a basis of possible fillings but must be experimentally established.

2. The particular manner of filling a gap which is adopted depends principally upon the amount (that is the number of

items or item groups) of information given to the observer, or constructed by him. There is a minimal amount of information below which nobody can fill the gap in the same way as others, and a maximum amount such that no normal person proceeding normally can fail to fill the gap in the same way as others.

3. When a gap is to be filled by interpolation through a step-sequence, the amount of information required to ensure uniform procedure increases rapidly with increase in the number of directions, or dimensions, in which the system concerned is simultaneously varying.

4. There may be objectively determined uniformity in number of steps, or in order of steps, or in both. Uniformity of number is more readily determined than uniformity of order. There are cases in which the only way of securing uniformity of order, or of number, or of both, is by command, or instruction. Whether, or to what degree such cases are properly brought under the operation of thinking remains for further discussion.

5. Once the manner of interpolation is selected every step normally increases the probability of some specific next step and at and beyond some stage short of the terminal point all steps are completely determined[1] as regards their possession of those properties which have to be regarded as pre-defined from the point of view of the system. Wherever, as in nearly all verbal series, the items have other properties as well, in respect of them the thinker operates with all degrees of freedom, and the system remains relatively, or completely, open.

6. There is a suggestion that intelligence may be related to the amount of information (items) required to achieve a gap-filling which is most uniform throughout the operator's cultural group. The most intelligent may be those who, with the smallest amount of information (items), produce that for which others need more information.

[1] It is perhaps permissible to run ahead of the evidence so far given, at this point, as a guard against likely criticism. The stage of complete determination may be reached either along a direct line of increasing probability for next moves or through a phase of increasing indeterminacy in which, a move having been made, the number of next moves that have legitimately to be considered becomes greater. The latter is very liable to occur, for example, within any system that is varying simultaneously in several dimensions. Sooner or later, however, so long as he is operating within a closed system, the thinker reaches a stage beyond which he is entitled to assert that he has achieved certainty.

CHAPTER THREE

Thinking Within Closed Systems—2

EXTRAPOLATION

1. SOME GENERAL POINTS ABOUT EXTRAPOLATION

If anybody who has agreed that thinking has something to do with the filling up of gaps is asked whether he considers the more characteristic thinking procedure to be one of interpolation or extrapolation, he is extremely likely to plump for the latter. For this view a number of different reasons are commonly given. Of these the two that seem to be the most interesting are:

1. "Thinking is a hard job anyway, or it ought to be; and extrapolation is more difficult than interpolation"; and
2. "Genuine thinking is always a process possessing direction. In interpolation the terminal point and at least some evidence about the way there are given, and all that has to be found is the rest of the way. In extrapolation what is provided is some evidence about the way; the rest of the way and the terminal point have to be discovered, or constructed. So it is in extrapolation that directional characters or properties are likely to become most prominent."

Perhaps the second of these reasons is only an extended form of the first. At any rate both of them are frequently given, and if they are accepted, it seems that a study of extrapolation situations may, in particular, help us to understand better the important directional aspect of the thought processes.

The move from interpolation to extrapolation brings us nearer to thinking as it most frequently occurs in experimental science, in daily life, and in artistic construction. In all of these the terminal points are more often than not included in what the

thinker must find. But there are also obvious differences, and as these are mostly concerned with the range of freedom within which the thinker operates at every stage, including, often enough, the move to the issue which is accepted as terminal, it seems a good plan to consider first what happens when extrapolation is required within closed systems. It must, however, be pointed out that as with interpolation, so with extrapolation; the only sense in which it can be said to be "simpler" when it takes place within closed systems, is that it is then more amenable to experimental test.

It is of some interest that extrapolation, though it is usually considered to make more difficult demands than interpolation, is clearly the more closely related of the two to skilled bodily performance. If there is any sense in saying that the body thinks when it is efficiently performing a complex skill, it is because in all such cases a few steps of a related sequence are given and the body is able to use what it can pick up from these few steps to shape further steps towards an issue that is accepted as fitting the objective requirements. Perhaps the main differences are that while the body is using movements which are extremely closely related to immediate perceptual stimulation, what is more often called thinking is using symbols requiring both immediate perception and longer range remembering, or perhaps long-range remembering only. Whether this constitutes a radical difference or whether all that can be said is that thinking achieves just the same things as complex bodily skill, but achieves them in a wider range of situations, may perhaps become clearer when we have considered a few experimental results.

2. AN EASY VERBAL EXTRAPOLATION

I shall begin by considering a straightforward illustration of verbal extrapolation. In this a rule of procedure, or, from the point of view of the items that are to be used, a rule of structure, must be extracted from incomplete evidence given, and the items which are to exemplify the rule are to be identified from among the total number of items given. No doubt it could be objected that the only part of all this which has any right to be called "thinking" is the extraction of a rule, and that once

this has been done all the rest is nothing but *identification.* It could be that identifying something which is available for perception all the time, is totally different from constructing a sequence of fitting items when none of them can be immediately perceived. However, let us first have a look at some of the results. The observer was given a card on which were the following words:

A, GATE, NO, I, DUTY, IN, CAT, BO, EAR,
O, TRAVEL, ERASE, BOTH, GET, HO, FATE.

E R A S E
F A T E

He was told "from the group of words at the top of the card complete the vertical arrangement indicated by the two words 'erase' and 'fate' taking 'erase' as the middle word in the column. Not all the words given need be used."

Although all observers were perfectly ready to complete a column of words, and appeared to be interested, very little uniformity of extrapolation was achieved. A few noticed that an initial E was followed by an initial F and made a list in alphabetical order, beginning with A and finishing with TRAVEL, including every word given. A few noticed that Erase had one more letter than Fate and made a list in which each successive word up to Erase had one letter more, and each word after Erase one letter less than its preceding one. So far as my results go about two people only out of every hundred taken at random and given the information already indicated, may be expected to produce

A
B O
C A T
D U T Y
E R A S E
F A T E
G E T
H O
I

Adding a third word item to the information given (thus
DUTY
ERASE) made much less difference than in the corresponding
FATE
interpolation instance,[1] but when another pair of items were given nearly all observers completed the series in a uniform manner. Since in this case the words available were limited in number and had merely to be selected from a given list, the suggestion is that the usual view that extrapolation is a more difficult method of gap-filling than interpolation is very likely well founded.

In this relatively simple case there appeared to be four types of extrapolation: (1) by a rule of alphabetical sequence; (2) by a rule of length of word sequence; (3) by a combination of (1) and (2); (4) by inserting words from the list given at random. Now (4) is, of course, just as truly a method of gap-filling as (1), (2), or (3), but it uses no identifiable "rule" or regularity, and those people who adopt it do not claim that the sequence which they adopt is any "better" or any "more necessary" than any other possible sequence. This raises a question which always crops up whenever thinking is experimentally approached by a study of gap-filling processes. Is (4), for example, as much an instance, or class of instance, of thinking as (1), (2), and (3); and are (1) and (2) on the same level of thinking as (3)? I will discuss this point later.[2]

Meanwhile, one thing becomes clear about the relation of the extraction of a rule to the application of a rule. Even when the items to which a rule must be applied are as few and as simple as they are in this case, it is still possible and even common to extract a rule from information given and then to make mistakes about the number, or the order, or the identity of the items within the system to which the rule is to be applied. For example, in this instance an alphabetical sequence could be adopted and then, after a few items had been filled in, a switch could be made to a word-length sequence, or even to taking words from the list at random. Or the word-length sequence could change into an alphabetical sequence, or a random selection.

[1] See p. 30.

[2] See pp. 72–5.

Even with material as unpromising as this, the urge to use words in a generalized, meaningful way is strong, and observers began with arrangements like

IN
I
TRAVEL

and then soon broke down into a mere random use of the words.

There was some indication that, when a series is changing, in more than one dimension, although more initial information is needed if there is to be uniform extraction of rules, once the regularities are extracted, the more dimensions of change are involved the less likely is it that there will be errors of application.

3. SOME MORE ABOUT TRANSFER

(*a*) *With Like Material*

It was easy to construct a number of word systems, or potential arrangements, after the manner of the one already considered. These could be given at any interval within the range of a single experimental sitting, which was from 90 to 120 minutes. As a matter of fact, from the point of view of transfer, it made no difference what was the length of intervening interval or what operations the observer had been required to perform during that interval. Observers who had succeeded in extracting one or more rule, or regularity of sequence, from their first example, whether immediately or only after a number of tries, at once searched for some corresponding, though perhaps different rule or rules, in each later example. Observers who had failed with their first example to make any but a random selection of items, proceeded to do the same thing again, and needed more than the ordinary number and order of items of information to jolt them out of this prepossession.

Another example was:

AFTERWARD, KIT, ENTRY, EFFORT, MANTLE, I, OVERTHROW, GAP, MOTOR, COST, OUTCOME, BET, COWSLIP, ENTER, QUICKSILVER, O, POTLUCK, QUIETNESS.

GAP

I

KIT

Any performer who realizes that an arrangement may be indicated in which each word, up to the middle I, has two letters less than its preceding word, and each word following the middle I two letters more than its preceding word, while at the same time initial letters, beginning with A, are two apart in alphabetical order, will achieve exactly the same list as any other performer who has extracted the same rules, except that he may have a free choice between certain words. In every case the procedure adopted with this example was immediately and effectively the one with which the earlier example of the same class had been left. This meant that anybody who had successfully extracted maximum possible regularities in the first example now needed no more than three items in the information column; but anybody who had not extracted any regularities in the first example, needed more items in the information column than he might have done if this had been his first example.

(*b*) *With Unlike Material*

The picture becomes very different if we use unlike material. An example of extrapolation completion which proved less easy than was anticipated was the following:

1234: 2134: 2143 . . . . . .

The observer was asked to continue to change the positions of the numbers in successive steps until he reached an arrangement at which it seemed "natural" or "sensible" to stop.

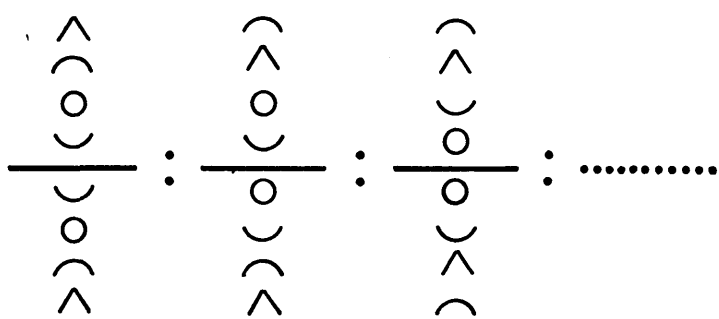
In the part of the system given, the first step reverses the first pair of numbers, and the second step reverses the last pair of numbers. It seemed very likely that a third step would be taken to reverse the middle pair of numbers, and then that precisely the same procedure would be repeated. This would reach the arrangement 4321, and it appeared likely that this would be accepted as the most satisfactory stopping point. The complete series would then run: 1234: 2134: 2143: 2413: 4213: 4231: 4321.

In fact most people confronted with this example simply went on changing the positions of numbers in a haphazard manner until they were tired, or until a halt was called. But an appreciable number did extrapolate by rule, using either the procedure indicated above or confining changes of position to first and last pairs only and producing

1234 : 2134 : 2143 : 1243 : 1234

as the complete series. The former regularity of sequence was about three times as likely to be adopted as the latter.

Immediately following on this example, the following was used, the observer again being asked to change the positions of the items in design, along the lines of the changes as indicated in the steps given, until by further steps he reached a "natural" or "sensible" final arrangement:



This case can be seen as precisely the same in principle as that of the number system just considered. It differs from the number groups because more items are involved, and (a matter of far greater importance) because the items are descriptively different. But the steps of change as given are the same as before. Above and below the line the first step reverses the positions of the first two items (reading from top to bottom), and the second step reverses the positions of the last two items.

No single person from among the very large number to whom I have given these two examples one after the other, has ever effected any transfer from the first to the second. It is true that those who had altered the positions of the numbers in a haphazard way, now changed the designs in a haphazard

way. But there was no admitted connexion from one to the other instance and people who had gone on playing about with number changes for quite a long time always stopped doing anything about the designs very quickly. In fact no observer, whatever he had done with the number series, ever treated the corresponding design series as in any way directional, except after much prompting.

Putting together with these results those already recorded for interpolation, some interesting indications emerge which must for the present be held tentatively only. If a rule of sequence is extracted from incomplete evidence as a result of the application of which steps of evidence given can be continued to an accepted terminal point, it is extremely likely that transfer will be at once and successfully made to other instances presented in material of the same descriptive character, even though the rule or rules involved in the new instances are different from the original ones. But if the new instances are given in items having a different descriptive character, even though the rule may be the same, transfer is unlikely without special prompting. However, it still remains uncertain to what extent the difference of descriptive character itself accounts for the lack of transfer, and to what extent the failure is due to pre-established methods of perception or interpretation. In the interpolation case considered, the main difficulty may have been chiefly due to an over-riding tendency to put words into some meaningful sequence. In the extrapolation case it may have been in the main due to an equally strong tendency to see each design given *as a whole*, and not as two groups each containing four items in sequence.

4. DIRECTION IN EXTRAPOLATION

Questions about directional properties and the various devices we are able to use in order to deal with them, can be raised whenever there is any thinking, no matter of what sort or at what level. As has already been suggested, however, it seems as if extrapolation situations ought to present a particularly clear case for a study of direction.

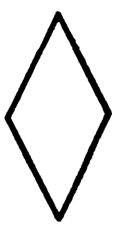
And so they do. For instance, even the relatively simple illustrations which have already been given bring out one

extremely important distinction about the ways in which directional characteristics can operate. Direction can be appreciated, or identified, by an analysis of those steps of evidence which are given and the extraction of a rule or regularity of sequence. Alternatively a direct leap may be made from the steps as given to a terminal point, and the missing steps then constructed with the already accepted issue in mind.

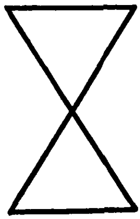
At this level of complexity, although the number of steps required are few and the "distance" to be travelled small, the analytical approach is preferred and is at least two or three times as likely as the "leap" or intuition. There were, however, a few people who consistently preferred the leap even if they had to make it at risk, and others who as consistently refrained from the leap even when it appeared to be clear that they could have made it.

Whether there comes a stage at which the number or complexity of the steps and the "distance" of the issue become predisposing influences prompting the method of the leap cannot be considered at the moment. It is already certain, however, that the ways in which directional characteristics can be identified and used depend, among other things, upon the subject-matter, or the medium of expression of the steps in evidence that are available.

With the numerical series there were a few observers who fixed at once upon 4321, or 1234 as the required terminal point. Usually they said they "saw" the issue. With the verbal extrapolations illustrated nobody ever claimed to "see," or in any other detailed way to fix, the individual words in any final arrangement. A few observers claimed that they "saw," or fixed, an outline shape, or frame, within which the word items must fit; thus.



in the first case, and

 in the second case.

So far then these cases must be distinguished according to which of the directional properties of the incomplete evidence are identified and used:

1. By an analysis of steps of evidence as presented, and movement through additional steps to the terminal point;
2. By a leap to ("intuition of") the terminal point itself and the movement from this to the required additional steps;
3. By a leap to ("intuition of") some scheme, or frame, or code, into which the detail of the terminal step must fit, and a subsequent movement to the additional steps.

It is obvious that both (2) and (3) are possible only when the steps of evidence as given are perceived—or somehow known—to possess some specific directional character. But the direction itself is undefined except in the sense of being exemplified in an alleged final step. That this can occur is certain; how it can occur is a matter for further discussion.[1]

(3) differs radically from (2). It sets up a frame, or a scheme, within which first the details of the final issue can be arranged and then from this arrangement the missing steps of evidence may be reconstructed. We cannot say that they *must* be reconstructed, because there are always some persons who, having made the leap, are satisfied with that. They do not want the evidence. Moreover there may be cases, though not at the level of these experiments, such that there are no known ways of reconstituting the missing steps, and yet the issue reached is widely accepted. It may even be that the person who makes the leap may never himself subsequently be able to reconstruct the required additional step: but some other persons have to do this.

The most important distinction between (1) and (2) is that

[1] See pp. 77–83; 151–8.

(2) requires the use of information which is not, at the time at which it is being used, directly accessible to sensory perception, but has been coded for storage.

Probably everybody will have noticed already that all these three forms of intuition (and the same must be true of any other that we may find) have the effect of turning extrapolation into interpolation situations. It may be that this is the chief practical advantage of the method of the jump.

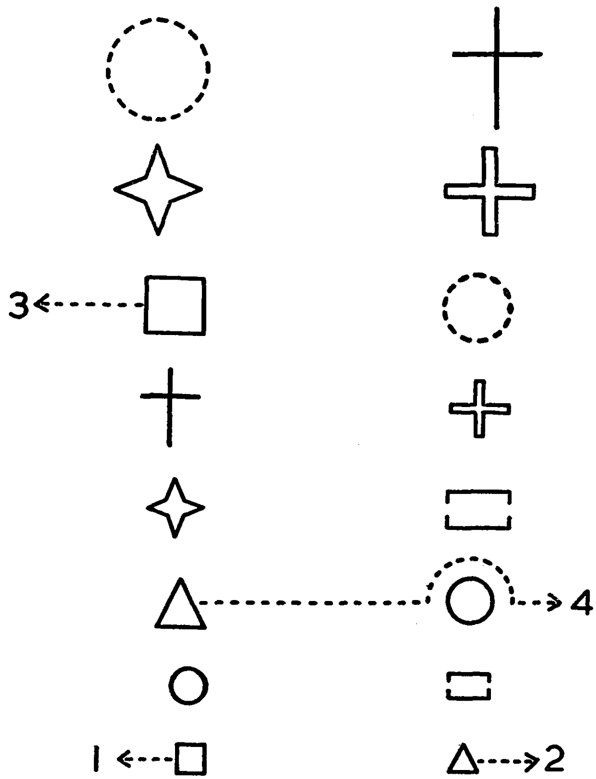
5. THE EXTRACTION OF RULES

Whether it is because nobody understands the process, or for some other reason, extrapolation by intuition is almost always regarded as the most interesting of the ways in which the direction and ultimate destination of evidence can be detected. The more common way is certainly by extraction of rules from steps of evidence which are given. So far as I know this process also has not yet been adequately described, or explained.

Perhaps the most widely accepted view is that rules for dealing with evidence are reached by much the same processes as those which are alleged to issue in classification and generalization. Numerous instances are held to be required, with overlap in some details and differences in others. Such combinations of common with individual features are supposed, of themselves, so to speak, to promote grouping, and it is further supposed that, if the grouped items have then to be treated in some way, resort will be had to a rule naturally and immediately. Whether, and if so in what sense, this view can be accepted, must be considered critically and by experiment. Rules obviously help to direct the treatment of evidence, but also the extraction of rules implies that evidence already possesses directional properties and that these are being utilized.

The best chance of getting a reasonable amount of experimental evidence without great delay is to take instances somewhat more complex than those so far considered, but not so

elaborate as to demand long-drawn-out courses of analysis. I therefore made up several cases of which the following may be taken as an instance:



The experimenter says: "This can be regarded as an arrangement of shapes which changes by a series of steps, as indicated by the dotted lines, into another arrangement of the same shapes. Pay regard to the evidence as given in the moves indicated and continue the process by steps until what you take to be a final arrangement is reached."

The full evidential indications will be seen to be:

1. Each move is outwards in a horizontal direction, and the length of moves increases from the bottom to the top.

2. No two shapes can occupy the same position, or, alternatively, all like shapes will ultimately be found on the same sides.

3. After the first two steps the *order* of move is from a smaller to the corresponding larger shape, the lower shape being moved before the upper one. It can also be taken that when a choice lies between two lower shapes the one in the left column has priority.

If these three "rules" are extracted and correctly applied the number, order, directions, and relative lengths of all the required additional moves are completely determined and are the same for everybody.

Within this system three variations are simultaneously to be controlled, determining (*a*) extent of move; (*b*) direction of move, and (*c*) order of move.

With only four steps of evidence as given not more than a minority in any random population can be expected to extract all these rules—so far as my own collected results go, about 8 per cent. A few others will extract all the rules, and then make mistakes in their application; but I have no cases of mistakes about extent or direction of move in any such instances; only about order of move, which, as before, seems to present the greatest difficulty. Increasing the steps of evidence given has exactly the same kind of results as have been discussed in connexion with interpolation, and again it is order determination that demands the greatest amount of information.

For the present, however, the question I am trying to answer concerns what sort of analysis will produce the desired rule extraction. We must look at the failures.

In the basic diagram the items display a number of points of agreement and some of difference, and it is certain that the extraction of rules must have something to do with these. An item, in this case, may be either a shape simply or a shape-together-with-the-dotted-line-and-the-arrow-head. There were a few people, a small minority, who took the shapes alone and classified them in some way, the commonest being to distinguish between closed and open figures. The basic diagram was

then rearranged according to the classification, e.g. all closed shapes appearing in one group and all open ones in another. Whatever the classification, this procedure leads to some final rearrangement which leaves a great amount to the whim of the individual. Shapes may appear in vertical or in horizontal columns, or any other way, and there is nothing compulsory about any particular order of appearance of individual shapes.

More often the items were taken to be the shapes in conjunction with the dotted lines, the arrow heads and the numbers appended. Observers looked for points of agreement and disagreement between shapes as treated. The great majority were satisfied if they found one point of agreement; some discovered two points and acted accordingly; as already recorded only a very few picked out all three points. It should be noticed now—for this is a matter likely to become very much more important when we move into more open systems—that the discovery of points of difference alone does not help much, but that of agreement does, or at least can. It will be clear that any observer who acted upon one only, or upon any combination of two of the points of agreement, was bound to be neglecting some of the available evidence, and to be leaving himself at various stages of his attempted rearrangement with more freedom than the austere exercise of thinking would readily tolerate.

Once more, as in various other contexts which have already been illustrated, it was the evidence that could be used to determine order of steps that was most frequently neglected. Even when the rule about order was extracted correctly, it was often applied wrongly, generally by missing out some step at the stage at which it should have been made. In instances of this kind, of course, a missed step can be inserted later, and no harm done, but in a great many more important kinds of instances a missed step means going off in an unfruitful direction altogether. To be able to settle order in sequence in a uniform manner seems to be a big feature of the appreciation of directional properties within the closed system. It also seems the most difficult feature in the sense that for the large majority of people it requires the greatest amount of evidence for its extraction as a form of rule. We may perhaps guess now that within the closed system discovery of order has somewhat the

same functions that accurate timing may have in less formalized settings.

6. SOME PRINCIPAL POINTS SUMMARIZED

These are some of the main statements that we may now provisionally make about thinking by extrapolation:

1. When a few steps of evidence only are given, their continuation to an accepted issue can be effected by the extraction from the items of evidence given of a rule (or rules) applicable to further steps yet to be made, *and* the identification of those items and that order of items to which the rule (or rules) are to be applied. Mistakes are most commonly made either by discovering some single rule and then neglecting others for which there is evidence and which must be brought into use simultaneously, or by missing out steps of application. Rules and regularities concerning order in sequence are the most difficult ones.

2. Another way of extrapolating is to make a direct leap from the evidence given to an accepted terminal point, and the missing steps are then constructed on the basis of the already accepted issue. On the whole this is less common than the analytic extraction of rules way. The leap, or intuition, may be to the terminal point itself, or it may be to some frame into which the detail, or items, of the final step must fit.

3. That a rule of sequence has been extracted, or a leap to a terminal point made, does not itself guarantee immediate transfer to other instances in which the same, or related, rules are applicable, or in which the same, or related, issues can be intuited.

4. The discovery of the directional properties of evidence, when this is based, as it most frequently is, upon the observation and analysis of steps of information as given depends upon:

(i) The selection of the appropriate items;

(ii) A search for *all* the points of agreement and difference among the selected items which are illustrated in the given steps of evidence;

(iii) A further selection of the greatest possible number of points of agreement and their combined systematic application to the selected items, in such a way that every successive step taken increases the empirical probabilities that the next step will be the same for all persons, at least up to such a place in the sequence at which probability drops out altogether because the next step or steps, literally, in an empirical sense, become the same for all.

5. The "rules" which are followed by extrapolation within the closed system concerned may be definitely formulated at the second of the stages indicated in (4), or they may be observed in practice at the third stage and never reach formulation at all.

6. At two stages, the first and the third, some process of "selection" has been introduced and this obviously will require further discussion. The experimental indications definitely are that the selection, whether of "appropriate" items, or of "appropriate" points of combined agreement and difference, always contains, as its key feature, some reference to the sequence, or step character, of the evidence as presented, and to the order in which steps must be taken. This means that there can be no sure and certain extrapolation (or any other kind of thinking for that matter) if only a single item of evidence is given. A "step" has significance for extrapolation only when it is related either to a preceding or to a succeeding step, or to both. One of the implications of this is that the method of the leap and that of the extraction of rules may be less radically different from one another than has often been supposed.

CHAPTER FOUR

Thinking Within Closed Systems—3

EVIDENCE IN DISGUISE

1. A GENERAL INTRODUCTION

There is a third class of situation in which information given has to be completed by bridging a gap in the attempt to reach some defined stopping-place. In these cases all the items needed are, in a descriptive sense, already present and available before the effort to use them begins to be made. But these items first have to be re-examined, from some special, and often unusual, point of view, and then re-interpreted. They are, so to speak, evidence in disguise.

Illustrations with which everybody will be familiar are the uses which frequently have to be made of scale drawing where, for example, what is represented in one scale must be interpreted in another; many of the requirements of map-reading; such operations as the building of a solid model from photographs taken of an object from different angles, and the interpretation of anagrams.

It may be that we are given, say, an air photograph, or an anagram, and at the same time a number of ground photographs, one of which is the precise counterpart of the view from the air; or a word list, from one member of which the given anagram has been constructed. All that may then be required is the identification of the correct ground photograph, or of the correct word. This may call for nothing more than a straightforward perceptual matching operation, and if so there perhaps need not be thinking at all, in the sense in which I am using the word.

In very many instances, however, there are no originals, or counterparts, with which the items as given can be compared;

or even if there are, merely identifying them correctly does not satisfy the requirements. There may be clues, some direct and some indirect, as when anagrams occur in a crossword puzzle and some of the cross clues have already been correctly assigned, or the clues without which the required reinterpretations cannot be made may themselves have to be discovered or constructed by some way of exploring the presented items. For example, we can have a number of photographs of an unknown solid object, and from these have to build a model of the object. Whenever more is required of evidence in disguise than straightforward identification, probably everybody would agree that the satisfaction of the requirements must involve thinking. It also seems initially likely that processes different from those which have already been considered under interpolation and extrapolation will need to be carried out. Whether this is the case or not the results of experiments should decide.

When all the evidence required is present to begin with, but in a form of disguise which must be penetrated, we get something that comes very near indeed to what has usually been called "problem solving." There is, in all such instances, an issue which can be regarded as "right," and a lot of others which can be regarded as "wrong." Yet the phrase "problem solving" seems to be definitely misleading. There is no psychological sense in saying that thinking takes place only if a "right" issue is reached. There is thinking whatever issue is reached so long as an attempt is made to carry further the evidence, or information, that has been made available. It has turned out that mistakes can be made in interpolation and extrapolation, and so they can in the processes of penetrating into a disguise.

Although we must not say that there is thinking only if a problem is solved or regarded as solved, it might well be the case that if the emphasis is going to be laid upon thinking as a form of high-level skill, somehow or other we must bring in the notion of efficiency, and we must be prepared to accept certain criteria of efficiency. This seems to me to be true, but still it is inconvenient to have to try to treat the solution of a problem, especially in any logical sense, as a criterion of efficient thinking. Sometimes, very likely, this can be done, but it may well

turn out to be the case that there are far more instances in which what is called efficient thinking opens up more questions than it closes.

Although, then, the sort of case which I shall now illustrate and discuss could be described as "problem solving"—and no doubt often has been—I believe it wise not to use that particular expression.

2. A CASE OF SIMPLE ARITHMETIC IN DISGUISE

$$\begin{array}{r} \text{DONALD} \\ \text{GERALD} \\ \hline \text{ROBERT} \end{array}$$

This is to be treated as an exercise in simple addition. All that is known is: (1) that D = 5; (2) that every number from 1–10 has its corresponding letter; (3) that each letter must be assigned a number different from that given for any other letter. The operation required is to find a number for each letter, stating the steps of the process and their order.

It will be apparent that all the items that must be used to reach the end result wanted are given, though not necessarily in the precise form in which they have to be used; and also that all the processes that must be followed are familiar and not difficult to any moderately educated person.

I will select a few of the attempts made to deal with this situation. In the first four the required terminus was reached, with little wandering. The steps and their order are indicated.

I

1. Given D = 5, then 5 + 5 = T	$\therefore$ T = 0
2. O + E = O $\therefore$ E must be either nought which is impossible or 9 and N + R > 10	E = 9
3. L + L + 1 (carried) = R $\therefore$ R is an odd number; but also D(5) + G = R, so R is 7 or 9, but E is 9	$\therefore$ R = 7

4. D(5) + G + 1 (carried) = R and R is 7 ∴ G = 1
5. A + A = E and E is 9 ∴ A = 4, and L + L > 10 A = 4
6. L + L + 1 (carried) = R and R is ~~9~~ 7 ∴ L = 8
7. N + 7 > 10 and as only 2, 3, 6 are now available ∴ N = 6
8. and B = 3
9. and O = 2

II

1. 5onal5 geral5 —— robert	5 + 5 = t ∴ t = 0
2. 5onal5 geral5 —— rober0	Either o + e = 0 (*a*) or o + e + 1 = 0 (*b*) (*a*) is impossible ∴ e + 1 = 0 ∴ e = 9
3. 5onal5 g9ral5 —— rob9r0	∴ a = 4 (one to carry from l + l)
4. 5on4l5 g9r4l5 —— rob9r0	Consider l + l. There is one to carry from 5 + 5, and also one to carry from l + l; ∴ l ⩾ 5 5 is bespoken ∴ l = 6, 7, or 8 If l = 6, r = 3; but 5 + g = r ∴ r ⩾ 7 ∴ r ≠ 3; If l = 7, r = 5, but 5 is bespoken ∴ l = 8 ∴ r = 7
5. 5on485 g97485 —— 7ob970	o + 9 + 1 = 0 ∴ there is 1 to carry from n + 7 ∴ n ⩾ 3; but as b ≠ 0, n > 3 ∴ (by exhaustion) n = 6

6.
```
 506485
 197485
 ------
 7ob970
```
$6 + 7 = 13 \therefore b = 3$
and (by exhaustion) o = 2

7.
```
 526485
 197485
 ------
 723970
```

III

1. $O + E = O \therefore$ O is zero or E is 9. But T is already zero $\therefore E = 9$.
2. If $E = 9$, A must be 4.
3. $5 + G = R \therefore R > 5$.
4. $L + L + 1 = 10 + R \therefore$ since R must be an odd number L must be 8 and R must be 7.
5. The result required can now be written down for all the remaining possibilities are limited and can easily be seen:

526485	or	DONALD
197485	or	GERALD
723970		ROBERT

IV

1. Since $D = 5 \therefore T = 0$
2. There is one to carry from the two D's $\therefore$ R must be an odd number.
3. Since $D + G = R$, R must be an odd number more than 5, i.e. 7 or 9.
4. Since $R = 7$ or 9, L must be 3 or 4.
5. Since $R = 7$ or 9, G must be 1, 2, 3 or 4.
6. Since $O + E$, or $O + E + 1 = 0$, E must be zero or 9. But T is zero $\therefore E = 9$.
   $\therefore R = 7$

7. If R = 7, then L = 3. But it cannot be, for A + A = 9. The error is in assuming that L = 3, or 4, when it might be 8 or 9. But E is 9 $\therefore$ L = 8
$\therefore$ A = 4
$\therefore$ G = 1

Since N + R must be more than ten and gives B, with 2, 3 and 6 left as possible numbers, N = 6
$\therefore$ B = 3
$\therefore$ O = 2

8. We have therefore

DONALD	=	526485
GERALD	=	197485
ROBERT	=	723970

The four successful and relatively straightforward attempts just given may be compared with another representative four, in which either the end sought was not reached at all, or it was reached only after a good deal of wandering about.

V

DONALD
GERALD D = 5, $\therefore$ T = 0

ROBERT

D = 5; O = 1, E = 9; L = 3; G = 2; A = 4.

Step 1. T = 0
2. L is less than T
3. G is less than 5
4. As G lies between 4 and 0 inclusive, R is 7, 8, or 9.
5. R cannot be 8, and if R is 7, L is 3.
6. But E = 9, and O = 1.
7. Then G must also be 1.
8. Perhaps E = 1 and O = 9

But G must still be 1 (Impossible!)

VI

DONALD	=	526485
GERALD	=	197485
ROBERT		723970

1. Tried putting letters in alphabetical order, against numbers 0–9 in numerical order: no good.
2. Wrote down every other letter, beginning with D and wrote the numbers from 5–0, 1–4 against them: no good.
3. Wrote down letters as they occurred in the problem against numbers in numerical order: no good.
4. Tried the letters and numbers by shifting the possible pairs: no good.
5. Wrote down the problem again with 5 in place of D, and 0 in place of T. No new ideas occurred to me so left the problem for several days.
6. Realized that there was no formula and for half an hour wrote in different numbers but failed.
7. Finally hit on a combination but it was not the right one for I had to have one more letter than was needed; e.g.

$$\begin{array}{r} 528465 \\ 793465 \\ \hline 1321930 \end{array}$$

8. A friend noticed what I was doing and told me that I had got O, E, and A right.
9. Wrote the problem down again, substituting the numbers known to be correct for the letters, and guessing the others until all that could not be correct were eliminated. Finally achieved the result as stated.

(This operator expressed the belief that he would have found the required result more quickly had he not been so eager to put in the figure three, this being his favourite number so that he wished it to occur more than once.)

VII

1. Substituted 5 for D.
2. Juggled with the figures 0–9, substituting until this stage was reached:

```
DONALD
5    345
GERALD
69   345
--------
9    690
```

3. Progress was now difficult, as there were O, N, G, B to equal 1, 2, 7 or 8. About ten minutes was spent trying to make these numbers fit in and as it proved impossible to do this the attempt was given up.
4. Decided to begin again with

```
DONALD
5    5
GERALD
     5
-------
     0
```

5. Wrote out a list of the letters with their corresponding numbers as known, thus:

A
B
D = 5
E
G
L
N
O
R
T = 0

6. Now realized that O + E had to give the O of ROBERT and so E had to be 9.

7. The numbers now seemed to fit in easily. They were identified in the order L = 8, A = 4, R = 7, G = 1.

In comment on the whole process this operator declared stage 3 to be the difficult one. It was hard to break away from an approach which nevertheless was leading to nothing definite. Once stage 6 was achieved the rest was "much easier," for all the numbers now seemed to "fit in."

VIII

1. If D = 5, T = 0.
2. Used trial and error, beginning from right-hand side. Decided the L could not be 5, or 0, or 7. Made no progress.
3. Tried beginning on left-hand side. Decided that as D = 5 G must be 1, 2, 3 or 4.
4. Suddenly saw that E must be 9 since this alone could become (1) O by having 1 carried from N + R, and O + E had to give O.
5. Now tried G = 1 and R = 7. They worked and the rest was quite straightforward.

These eight attempts are fairly representative of a very large number collected, except that the proportion of failures to reach the required end-place was rather greater than is suggested. A few general comments are called for at this stage.

3. SOME POINTS FOR DISCUSSION

(*a*) *The Steps in the Sequence*

In all the examples given the steps and their order are as they were recorded by the persons attempting the experiment. It will be noticed at once that in this case there is a greater range of interpretation of what constitutes a "step" than in the direct interpolation and extrapolation processes so far discussed.

In the first four examples a "step" is treated as any process which is directly implicated in a preceding process, and directly introduces a succeeding process. So what is treated as a single step may itself have to be set out in a serial manner as with I, 3;

II, 4; III, 4 and 5, and IV, 6. If, however, VI, VII, and VIII of the second group of examples are considered, we find that "step" has become, not so much a move, or a set of moves, in a sequence regarded as inevitable, as a description of one of a succession of varying approaches to the operation required. The "step" may then include all sorts of things which are not specified at all, as in VI, 4, VII, 2, and VIII, 2.

Examples I, II, and III all make immediate transition from the right-hand column of the addition to the second column from the extreme left, and establish that E must be 9. IV—introduced here as an example of an immediately corrected mistake—proceeds effectively only when, in steps 6 and 7, E is proved to be 9 and this evidence is used. V reaches no termination, but VI, VII, VIII, however they got to their stopping-place, do so only after they have decided that E = 9 and have applied this decision.

It seems that in this case to establish E = 9 is a key step.[1] Once that is demonstrated, everything else can be economically presented in a form of moves having an internal and necessary connexion. Until that has been shown, the only procedure available is the trial one after another of as many different possible avenues of approach as the person who explores them can stand making. Maybe this is what always happens if evidence in disguise has to be extended to reach a desired stopping-place.

In direct interpolation and extrapolation the number of steps needed to fill a gap was easier to determine—i.e. required fewer items of information—than the order of those steps. Although in this instance of evidence in disguise I have not varied the amount of information initially supplied in any way, it seems at least highly probable that the same is true. It will be seen that the number of assigned steps in the first four examples varies from 5 to 9 inclusive. But most of this variation is due to differences of interpretation about what can be called a single step. If any one interpretation of "step" is adopted, the possible variation in number of steps to be assigned becomes small. But, once the key step is taken there may still be quite

[1] Compare *The Bearing of Experimental Psychology upon Human Skilled Performance*, Brit. J. Indust. Med., Vol. 8, 1951, p. 214.

a lot of variation of route to the same terminal point. In this particular illustration it makes little difference which of several routes is followed, provided the first step taken is $E = 9$, but clearly there must be plenty of cases in which the order of steps does make a great difference to the probability of final arrival.

(*b*) *Transfer Again*

It is obvious that anybody who is able to deal at all with evidence in disguise must be able to make a transfer of training already received to the situation which is now presented. But whether the transfer is helpful or harmful is another question. The way things turned out with this example suggested very strongly indeed that the transfer was helpful only when the disguise, and some characteristic instance of the situation that is disguised, could be related, each of the two situations at the same time retaining its own special structural features.

Miss M. Skinner, commenting upon a number of attempts to deal with the Donald–Gerald case, which she collected for me, said: "Several more students tried the problem, but couldn't do it. They substituted 5 for D and zero for T, but since no direct clue is provided for L and R, they said they couldn't get any farther. It seems that the habit of starting to make an addition sum from the right-hand column and continuing to the left with succeeding columns was so deeply ingrained that they couldn't conceive of any other method of approach and they soon tired of trying to find L and R by trial and error."

Similarly in examples VII and VIII as recorded, nothing much is achieved so long as the letter combinations are treated precisely as if they were a straightforward simple addition sum, and the clues available from the repetition, or the special relations, of particular letters ignored. But when what has been formerly learned about simple addition is combined with what can now be learned from the structure of the name forms, all goes well.

There are, of course, plenty of instances in which all that is called for is a rescaling, or a rephrasing, or a literal translation. Then, provided that the training a person has received has given him the necessary information in an accessible form direct transfer can be made, and is extremely likely to be made.

All that may be needed is the extraction and application of a rule, or simply the application of a rule given as a part of the initial information, such as, for instance, of an indicated scale, or some other regularly adjustable feature of a required redrawing.

Translation from or into a foreign language is an interesting case of dealing with information in disguise. If there is a generally acceptable literal equivalent in the language required for every word, or phase group, in the language given; provided the translator can remember, or look up the words or phrase groups concerned, he can make an effective transfer. All that he needs to do is to *identify* the words and phrases satisfactorily, and once more the question crops up of whether such identification, or matching, is to be regarded as an essential part of thinking.[1] Very often, however, there are no literal counterparts in one language for words, or phrases, in another. The position, then, is exactly that of our DONALD + GERALD instance, except that the translator usually works with a far greater range of freedom. An acceptable translation means that the translator must move from one language system to another, keeping in mind all the time the structure and genius of both languages. Judging from the large proportion of people who failed to reach the desired issue in the relatively simple DONALD + GERALD example, it seems that this sort of transfer presents considerable difficulty.

(*c*) *Guess or Intuition*

One of the remarkable things about this case is the frequency with which steps in the series which, in theory could, and perhaps should, be stated, are not mentioned. This might happen, of course, merely because the person concerned wanted to avoid the bother of writing them out. In the great majority of instances, however, it was clear that this was not the reason and that the step or steps unexpressed were not, in fact, definitely worked out at all. Compare examples II and III. In II, produced by a Cambridge graduate in mathematics, every step is meticulously described; in III nothing is developed after step 4. In IV apparently the values of B and O are guessed or

[1] See also pp. 150–1; 191–5.

"seen". In VI O, E, and A values having become known, it is claimed that all the others are "guessed." In VII there is still more guessing ("seeing") for after E has been shown to be 9, "the numbers now seemed to fit in easily." In VIII also, as soon as E is "seen" to be 9, it is claimed that specific values were tried for G and R, and "the rest was quite straightforward."

Already, in considering how the directional character of evidence, where forward extrapolation is required, comes to be appreciated, I have given some elementary examples in which "leaps" replaced "steps," the terminal point being reached before the route to it has been, so to speak, plotted. It is now clear that a jump may be as readily made to a step, or to some of the steps, on the way to what constitutes the stopping-place, as to the stopping-place itself. In this particular example, indeed, I have never known anybody claim to leap direct from the information given to the full reinterpretation required. At the least, what I have called the key-move must first be made, and this can be done either with the help of a detailed analysis or by some kind of process which is described as "seeing it."

So far, then, there seems to be evidence of three distinguishable processes by which information can be developed, through, or without, a detailed series of steps, to a conclusion, whether by interpolation, extrapolation, or reinterpretation. There is an analytical process in which all the moves are formulated and each successive move becomes empirically more probable as progress continues. There is sometimes a leap to the conclusion, so that no moves are formulated until after this conclusion has been reached. Then sometimes the moves are specifically indicated, and it appears that within the range of thinking with which we have been concerned so far, it can be always assumed that this might be done. Thirdly, there is a mixed process in which some moves are made as a result of specific analysis, others are then "guessed," and the conclusion is finally achieved although perhaps only one or two of the moves have been clearly justified. For the present I intend to follow the usage which was regularly adopted by the people who tried my experiments. I shall take "guessing" to refer to the acceptance of steps, or moves, to a conclusion, without assigned justification, and "insight" or "intuition" to refer to the acceptance of a

conclusion without assigned steps. Whether this is any more than a terminological convenience may, I hope, become clearer later.[1]

Meanwhile it is extremely important to realize that at this level "guessing" *never* is random. In every case some evidence is available, and if there is thinking some evidence is being used. No person who claimed to "guess" numbers for letters in the DONALD + GERALD example, ever worked in a random way through all possible letter-number combinations. Even those who may have imagined that they were doing this, but got tired of it, and gave up, long before they had completed the game, naturally accepted 5 as belonging to D and 10 as belonging to T, and both by exclusion, to no other letter. Also they realized that this carried some positive implications, and though they did not work out what these were, the failures were far more due to prepossessions about specific numbers (as in example VII given) and number-letter combinations than to getting bored with a prolonged process of random trials.

(*d*) *Mistakes*

In the examples given IV, step 7, is an instance of a mistake which is immediately realized as wrong, and is at once corrected. In V, steps 7 and 8 are instances of a mistake which is at once known as an error, but it is not corrected. Knowing that a mistake has been made may merely lead to a deadlock.

While this experiment does not show clearly and decisively what are the psychological differences between knowing that an error has been made and correcting the error, it does suggest one extremely interesting difference which might be further experimentally studied. In both cases the "direction" of the process, up to and beyond the point at which error occurs, is appreciated. When a mistake is corrected in the manner illustrated in IV, however, it is next steps, or anticipated steps, that have the greatest influence, and these are not limited by any preformed decision of a generalized kind. It will be noticed that both IV and V get confused at the same point and by precisely the same assumption, i.e. that L = 3, but V has already inflexibly decided that L must equal 3, whereas IV,

[1] Cf. pp. 180–2.

having made no such decision, is ready to try other possibilities. By V the system has been closed up at too early a stage and alternatives that still remain possible fail to be considered. The preliminary allocation of numbers to letters is acting exactly like an uncritically accepted generalization, which, once it is made, may stop any more questions from being asked.

4. VERBAL DISGUISES

(a) *Anagrams and Cross Word Clues*

Some common forms of verbal disguise concern single words and word-groups, and others require a reshaping of whole word passages. In the first class are anagrams, and many of the other devices used by the maker, and unmasked—if he is successful—by the solver of crosswords. Penetrating the disguise of single words takes one or other of two general forms: (1) the required reshaping is done at a jump and apparently without any mediating steps; (2) letter items are built up by questions, or by using cross clues, and the reshaping is achieved through a number of steps.

If the second method is being used, there are certain things that nearly everybody does in a fairly constant order. When all the evidence is available for a complete word, but is in a disguised form, the first demand is for the number of letters in the word. This is an introductory step towards making the situation one of extrapolation or interpolation; it provides a terminus though descriptively the terminus still remains unspecified. The second step is to search for the first letter, and after this either for the terminal letter, or, if the number of letters is more than four, for the third letter, and so on, usually taking letters next but one in the order left to right. If additional clues can be discovered, as by the solver of a crossword puzzle, they will be taken in any order found easiest by the solver. But the general attempt all the time is the same; once having got the situation into a form requiring extrapolation or interpolation, to try to proceed by that sequence of steps which progressively diminishes the number of probable next moves.

Once this stage has been reached, everything that has already

been said about interpolation and extrapolation becomes applicable to this special case of disguised evidence. The "extraction of rules" that is involved, however, depends upon some utilization of the structure of whatever language is being employed. It is important to realize, as well as difficult to understand, that the capacity to make effective use of language structure does not in any way demand a detailed knowledge of letter frequencies or letter combination frequencies in the language concerned.

Every cross-word addict will be familiar with the experience of getting required words, or groups of words, in what seems to be a single jump. The most curious instances are those in which the disguise is successfully penetrated, but at the time the solver is completely unable to say why the result is correct. For example, take the clue[1] "This capital sounds as if it were multiplying." No doubt people could be found who would give the required name at once, but it is far more likely that the usual first question will be asked: "How many letters?" The answer is "five." The search then goes on until initial, final, and perhaps middle letters have been given. These are I. I. H. At any of these stages, but with increasing frequency with increase of information, the solver is likely to say "It must be IRISH," and is extremely likely to add "But I can't see why." Later, often when they are doing something different, a good many solvers suddenly say "Well, of course, the Irish capital is Dublin." Some never get as far as this, yet still remain perfectly satisfied with their solution.

In this instance, steps (constituent letters) towards the required terminal word require to be established in almost all cases. Everybody must be familiar, however, with numerous instances in which, especially in the case of straightforward anagram forms, no steps whatever appear to be assigned. For example "ARE WE, LAD, IN THE RIGHT STATE?" was given as a clue in a crossword, with all the statements for possible clues provided.[2] In rather more than half the number of instances collected in this case, all from solvers at University level, DELAWARE was written, sometimes at once, some-

[1] Taken from a *Daily Telegraph* crossword puzzle.

[2] This was taken from *The Times* crossword puzzle No. 7945.

times after delay, and no steps assigned. Some solvers made comments as:

(i) "The answer suddenly 'clicked' ";
(ii) "DELAWARE occurred 'in a flash' ";
(iii) "DELAWARE just came!"
(iv) "The first three words being somewhat unusual suggest an anagram, which immediately rearranges itself as given";
(v) "Two days ago someone asked me if there was a State of this name. Therefore one look and it was solved";
(vi) "The word 'lad' seemed unnecessary: try anagram: fits DELAWARE."
(vii) "The letters fell into place at once."

It is clear enough, from these statements, and from many others made concerning this or comparable examples, that although no steps to a terminus are assigned, evidence is in fact being used all the time, and may be being used in a sequential manner. It is also clear enough that the evidence that is utilized consists in part of information that is, so to speak, "taken out of store," and that the storage must be of a very highly flexible nature.

The first thing to happen, as a rule, is for the solver to try to determine what sort of a situation has to be dealt with, to assign a class, or a category: "this is an anagram." Any combination of words seen, or of sounds heard, as unusual, or any dominant word or sound treated as out of place, not fitting, could, in this instance, be employed to indicate the category "anagram." But when the circumstances are changed, precisely the same features of the immediate evidence can be used to suggest a different label. What now is called an "anagram," for instance, then may be called a "code" or a cryptogram, though the first may require only a rearrangement of the letters, whereas the second may demand a complete new outfit of letters. Not only can the same immediate evidence be used to lead to different characterizations, but, as the remarks just noted about the Delaware instance show, different immediate evidence may lead to the same characterization. It is obvious that something is coming from information stored, and whether

this information is stored in a final and unchanging form or not, it is certainly not being used in such a form. It also seems clear enough that what is coming from store and the character of its relationship to the immediate evidence, are both very strongly influenced by the circumstances of the moment. These, it appears, play a predominant part in the assignment of a label for the evidence, and thereafter in developing its directional properties.

(*b*) *Continuous Passages*

It frequently happens that continuous, meaningful verbal compositions are presented in such a form that they need reinterpretation and cases of this now had to be considered. Possible instances seemed to be: translation from one language, or idiom, to another; decoding of messages; providing a gloss or an exegesis of difficult, mystical or symbolic writings. I considered all of these but rejected them. In the first place they seemed to require operations of a relatively technical and specialized nature and all of them have often been set in a frame of controversy which seemed out of place for my purpose. Secondly, it appeared that the form achieved would most likely be little more than a sort of "copy" of the original, a case once again of "matching" rather than of "thinking."

I then wondered whether it might be possible to choose some brief general argument, and require it to be reshaped, so that a form of the same information could be used to arrive at a different terminus. I therefore adapted a quotation from *Rasselas* which I remembered to have been set many years ago at a university examination:

"Anybody who has a lot of things to do is sure to do something wrong, and for this he will have to suffer the consequences. Even if it were possible that he should always act rightly, a large number of people will be watching him and judging his conduct, and the bad ones among them will blame him and try to hinder him because of their ill-will; while the good ones are sure to make mistakes sometimes and then they will condemn him unjustly. People who hold important positions, therefore, cannot hope to be happy."

All persons who tried to deal with this were given the

passage to read and kept it before them while they followed the instructions (*a*) Keep the form of this argument the same, but rewrite it, so that you can come to some conclusion about "men in humble" positions; *or* (*b*) Without altering the sense of this passage rewrite it so that it comes to some conclusion about "men in humble positions."

It must be admitted at once that, considered in the present context, this was not a particularly successful experiment. The most interesting result was, in fact, that treatment of this passage was constantly breaking out of anything that could be called a closed system. Already we have seen that, unless special precautions are taken, whenever words are used, there tends to remain, at all stages of their treatment, a relatively large amount of freedom. In general, while thinking *process* hankers after the closed system, the thinker himself revolts against it whenever he can.

No consistent differences emerged between responses to instruction (*a*) and those to instruction (*b*). If the same person dealt with both (*a*) and (*b*) he would usually elaborate whichever was his second attempt giving, as a rule, some additional illustration to fortify whatever conclusion he favoured.

About one-third of the attempts concluded that "men in humble positions cannot hope to be happy," while the other two-thirds concluded that "men in humble positions can hope for happiness," or "can hope to be happier than others."

Many of the versions produced resorted to argument about the original, or introduced any new material which suggested itself as supporting the conclusion adopted.

Here, for example, is one instance:

"The instructions are misleading, for the paragraph as it stands is nonsense. Let us analyse it sentence by sentence:

"1. The busy man will make mistakes and suffer;

"2. Even if he makes no mistakes, he will be blamed and hindered;

"3. 'Good' people make mistakes and condemn the busy man —if they do this, how can they be 'good'?

"4. Therefore people who hold important positions can't be happy.

"This doesn't follow at all, unless 'busy' is synonymous with

'important.' I should leave the paragraph exactly as it stands, but change the last line to 'People who hold humble positions, therefore, cannot hope to be happy.'

" 'Humble' people are more likely to be 'busy' than 'important' people."

And here are two others:

"Persons who write a great deal are sure to write some nonsense and for doing so will be rewarded. Even if they never produce any nonsense, in moods of elation they will believe all their work is nonsense, and when depressed some of their friends will endeavour to cheer them up by seeing unreason in their veriest profundities. Men in humble positions therefore cannot hope to be happy."

"Men in humble positions cannot hope to be happy because people in good positions have more chances of acquiring the better comforts of life, such as cars and really modern homes. Secondly, the humble man cannot hope for certain positions in life without a sound education which, of course, requires a large amount of money. Thirdly, he naturally resents the person in a good position, because he knows he cannot have that position himself. Finally, therefore, no matter which way we look at life, the humble man just cannot be happy."

To pretend that any of these attempts—and there is nothing unusual about them—represent thinking within a closed system as I have defined it, would be nonsense. It is true that they move, as all other collected instances do, to a final and predetermined terminal statement: "Men in humble positions cannot be happy." But on the way to this issue, almost anything that can be given a form judged admissible can be introduced, and there is no approach to uniformity in the number or the order of the steps.

Very rarely the instructions given were treated in a strict and literal way. Then, for every statement made in the original, an equivalent form was sought, but with "people in humble positions" or "who have little to do" as the grammatical subject. The versions then produced were highly uniform. An instance is:

"Anybody who has very few things to do is sure to do something right, and for this the consequences may be fortunate.

Even if it were possible that he should always act wrongly, a small number of people will be watching him and judging his conduct, and the good ones among these will praise him and try to help him because of their good will; while the bad ones are sure to be correct sometimes, and then they will praise him though he does not deserve it. Men in humble positions, therefore, have a good chance of happiness."

In all such versions, however, the number and orders of steps in the argument and the assertions made at each stage are not really being treated as following one from another. They are successions copied—once decision has been made concerning the form required by the instructions—from the original; they are not sequences in the proper sense of the term. In fact there seems to be thinking in this kind of case only when the material is treated with freedom, and then the thinker has broken out of all closed systems and his effort falls rather for consideration under what I later call "everyday thinking."

Obviously it would be possible to go very much farther with the study of various other types of instance which required some reshaping of evidence expressed in continuous meaningful compositions. For example, scrambled writing might be used which would have to be reshaped in a normal sequential form. Or we might try to look into the many interesting questions which arise when composition in one mode has to be expressed in a different mode. The present study, however, makes no pretence to completeness, and a stage seems now to have been reached when some attempt to develop a theory of thinking within the closed system must be made.

5. SOME MAIN POINTS SUMMARIZED

1. Thinking may have to deal with evidence in disguise. This is the case in which all the information needed is present, but the items have to be examined from a special, and perhaps unusual point of view, and then reinterpreted or rearranged.

2. A case is considered in which letters have to be assigned numbers in such a way that when two familiar Christian names can be treated as added together the result will be a third

familiar name. One letter only is assigned a number. Some letters are repeated. Thus:

$$\begin{array}{c} \text{DONALD} \\ \text{GERALD} \\ \hline \text{ROBERT} \end{array}$$

$$D = 5$$

3. While there is no reason to doubt that in such cases as this one the required reshaping of information could be accomplished "all at once," this is exceedingly rare, and in all the instances collected steps were assigned in an order generally treated as internally determined.

4. No person who assigned numbers to letters in a relatively haphazard manner ("by guess") reached a correct issue. There was also no instance in which a guessing procedure came anywhere near exhausting all possible combinations.

5. Nobody who tried a conventional simple addition procedure, starting from the extreme right hand and moving regularly leftwards, reached the required issue. Successful transfer required that what had been learned about addition must be combined with what was directly available from observation of doubling of letters and the like.

6. The importance of the notion of "Key move" (in this case that E = 9) was established, and once the "Key move" was made "leaps" often replaced "steps."

7. To identify an error did not ensure the correction of that error.

8. (*a*) Anagrams and crossword clues, and (*b*) the reshaping of an argument were considered as instances of verbal disguise.

With (*a*) when a step method was employed the main attempt was to get the situation into a regular extrapolation or interpolation form and then to proceed in the ways discussed in earlier chapers. More frequently than in other types of instance no detailed steps were assigned but a terminus was reached in a jump, and very often recognized as fitting only some time later. In all such cases evidence was used, though not stated, and some of it was taken from memory (or storage).

This "stored" information was normally highly flexible in form and function.

In (*b*) cases it was common for reshaped passages to contain items, or arguments, or criticisms not in any sense present in the original, varying greatly from instance to instance, and inconsistent with any "closed system" operation.

CHAPTER FIVE

Theoretical

1. THE USE OF THE WORD "THINKING"

By now it will be abundantly plain that I am using the term "thinking" in a more restricted and technical sense than is convenient in everyday conversation. Either this must be done or else some other word or phrase must be found to describe the sorts of processes which I have illustrated and shall proceed to discuss. For as we employ the word in rapid talk "thinking" can be used to refer to almost any mental process that runs beyond immediate perception. "Sorry," we say, "but I didn't *think* of it," when we mean that we have forgotten something; or "Oh yes! I *thought* of it," when we mean that we remembered something. "To think" may be synonymous with "to believe," as when we say: "Well I *think* that he is a reliable person"; or precisely the same expression, with a difference of inflexion, may indicate "doubt." We can even use the word "think" when we want to indicate that a mistake has been made: "I'm sorry, but that is what I *thought*, anyway"; or we may use it so as to imply that *if* there had been thinking no mistake could have been made: "Why didn't you *think* instead of just jumping to a conclusion." In the most general sense of all "thinking" is any process of "occurring to mind," as we say, and "a thought" is whatever enables a human being to refer to something which is not regarded as an immediate part of the external environment. If we try to look for some character which is common to all of these processes, and to many others to which the term "thinking" is loosely applied, we shall find one only. There is something in all of them which cannot be wholly accounted for in terms of a response to an immediate external environment. Many psychologists have expressed views which imply that this characteristic of being able to add something to

whatever can be learned from the environment of the moment is a sufficient, as well as the most general mark, or sign, of thinking. Professor D. O. Hebb has made perhaps the most explicit statement of all recent psychological writers. He is discussing *The Organization of Behaviour*. He says that the "central problem," considered from a psychological standpoint, is "the problem of thought: some sort of process that is not fully controlled by environmental stimulation, and yet co-operates closely with that simulation." "The failure of psychology," he continues, "to handle thought adequately . . . has been the essential weakness of modern psychological theory."[1]

Naturally, I should not wish to deny that this is a possible view. What it really urges, however, is that all psychological problems of a cognitive character are basically problems of thinking. For remembering, and forgetting, and believing, and doubting, and making mistakes, and using evidence correctly, and many other processes that psychologists have to distinguish, are all instances in behaviour of processes which, though not fully controlled by environmental stimulation, yet co-operate closely with that stimulation.

Beyond a doubt it is possible, and interesting, to discuss in a general and speculative way what kinds of notions must be used about organized behaviour, or about its underlying physiological processes, if we want to try to understand how direct sensorial and perceptual evidence can be supplemented. But the moment we endeavour to make such discussion concrete we shall find it necessary to accept further distinctions.

For example, not very long ago, after an absence of a little more than seven years, I revisited a large American town. Much of its building and many of its other characteristics had changed during the interval. However, as anybody else would have done, I at once remembered a good many of the things that had happened to me, and that I had discovered on my former visit. On this occasion, I had to find a place which I had not previously visited, and I had no town plan. What I

[1] Hebb, D. O.: *The Organization of Behaviour: A Neuropsychological Theory*, p. xvi (1949, London: Chapman and Hall Ltd.; New York: John Wiley and Sons).

could remember was a help, but it was not enough. I had to use what I knew in general about the layout of American towns, combined with information which I had been given about what to look for in this case, and with cues of various kinds which I could pick up from the current scene. I reached the spot I wanted to find, and I am sure that any other rational person seeking the same terminal point, and having the same information available, would have used it in the same manner to arrive at the same end. Obviously there was something both more and less than specific remembering in all this. Stored information was being used, but its temporal position in relation to other information, stored or immediate, was of no importance. In this respect there was less than specific recall. Also current information was being supplemented from various sources of stored information to reach a terminus which neither the information of immediate observation nor that drawn from store, could by itself achieve or describe. In this respect there was more than specific recall.

The use of any contributory sources of evidence that are available to reach a terminal point which is treated as if it had not been achieved before seems as if it must be regarded as particularly characteristic of thinking. It is important to consider the phrase "treated as if it had not been achieved before." Probably in the vast majority of cases it has been achieved before by somebody else, and in a great many it has been already reached by the very person who now reaches it again. The point is that the terminal point is treated as if it were inherent in, necessitated by, the evidence; it is never merely recalled as having been achieved before. Thinking, that is to say is, in my use of the word, not simply the description, either by perception or by recall, of something which is there, it is the use of information about something present, to get somewhere else.

If this is accepted, there is always in thinking a possibility of a succession, or a series, of interconnected steps. As we have already found, in many instances, these steps are not articulated, or formulated in any way. But if a jump is made from information given, accepted, and describable to an alleged terminal point, it is invariably assumed that intermediate interconnected

steps can be found, even in those cases where so far they have not been found. It is this which confers upon all thinking a character of necessity, or compulsion.

The important characteristics of thinking process, as I am proposing to treat it, can now be stated:

The process begins when evidence or information is available which is treated as possessing gaps, or as being incomplete. The gaps are then filled up, or that part of the information which is incomplete is completed. This is done by an extension or supplementation of the evidence, which remains in accord with the evidence (or claims to do so), but carries it further by utilizing other sources of information besides those which started the whole process going, and, in many instances, in addition to those which can be directly identified in the external surroundings. Between the initial information and the terminal stage, when the gaps are alleged to be filled, or completeness achieved, theoretically there are always a succession of interconnected steps. These steps may be described either before or after the terminal point is reached. They, more than anything else, are what makes an experimental approach to thinking possible, and they confer upon thinking its character of necessity. We are not to suppose that this necessity implies that, given the initial information, the steps through which a terminus is reached must always be the same steps, or the same number or order of steps; or that the terminal point reached must be the same in all cases.

More briefly thinking can be defined as: The extension of evidence in accord with that evidence so as to fill up gaps in the evidence: and this is done by moving through a succession of interconnected steps which may be stated at the time, or left till later to be stated.

If anybody should now wish to say that what I have defined is only a special case of thinking, is perhaps what in daily talk is often called "real, hard thinking," I should make no objection. At any rate it is this that I have been illustrating and am now going to consider theoretically, in so far as it can occur within a closed system.

2. THE CHARACTER OF DIRECTION

To say that thinking always has a character of direction is, at the level of our present discussion, to make a merely formal statement. The definition of thinking already given itself implies that in any specific case the process moves from its start to its finish with a kind of necessity. This, however, still leaves open what variety of conditions can set the move going.

The basis of all human observation is to be found in the special senses. By their nature and the long course of their development these are successful as much because they fail to pay regard to some environmental features as because they are able to pick out and respond appropriately to others. Some, at least, of what is sensed or perceived, is stored and becomes available for later use. There appears to be plenty of evidence that what is so stored can undergo a great amount of elaboration and change, so that when stored information is used together with current information—as, in thinking processes it has to be—the information remembered need not possess the same gaps or incompleteness that it had when it was first appreciated. Usually, however, probably always, especially after long-term storage, information drawn from this source has gaps of its own, due to forgetting and other processes.[1] Whether, then, we are dealing with evidence from immediate perception, or from storage, or from both, this evidence cannot escape from being fragmentary, and having gaps.

But we are certainly not entitled to assume that the occurrence of gaps in evidence, or even the appreciation that gaps are present, necessarily leads to any attempt to fill up the gaps by that kind of process which I have been characterizing in the preceding section of this chapter. If that were the case everybody would be thinking at every moment of his waking or dreaming life. In fact, nobody claims, or ever has claimed, that he does that. Moreover, every one of the experimental approaches which we have been considering has shown that very diverse gap fillings and completion processes may be produced by evidence which, in an objective sense, does not vary from case

[1] See, for example, Chapter X in *Remembering* (Cambridge University Press, 1954) or Chapter V in *The Mind at Work and Play* (Allen and Unwin, 1951).

to case or person to person. This also suggests strongly that something more lies behind the directional character of thinking than simply the perception of gaps and incompleteness.

If we look over our experimental results we find that two things have turned up again and again. The first is that there is a minimum amount of evidence at or below which any extension of that evidence is a matter of individual idiosyncracy. The second is that when more than that minimum of evidence is available, still the ways in which gaps are filled are very much smaller in number than the ways in which they might theoretically be filled. I have already accepted the view that any process which can be called thinking is to some degree at least constrained. Information which is so treated that a person is equally likely or able to strike off from it on any one of a theoretically unrestricted number of different ways according to his whim of the moment, is not in the form which thinking demands. This is what is meant when we say that strictly single item information cannot initiate a thought process. There must be at least two items, and there may have to be many more, especially when a series is changing simultaneously in different dimensions. Further than this, the thinking person must respond to these items as linked together in some way, or ways, and he must submit to yet more constraint by going on to treat them as constituent items in some system with rules or conventions.

All of these are features of evidence which are necessary if there is to be thinking, but they are not by themselves enough to make certain that thinking will take place. There seem to be three basic questions which must be considered: (1) By what kind of mechanism are we able to appreciate the directional properties of evidence? (2) By what processes can we suppose that thinking actually goes to work arranging items of evidence into the sequences that are appreciated as possessing directional properties? (3) What are the processes by which the items of evidence that are to be put into such sequences are selected and arranged?

(*a*) *Spotting Directional Properties*

There are no doubt some problems of thinking processes, as I have defined them, which are to be found only at the high

level of thinking itself. But the question of how we come to be able to detect the directional properties of evidence is certainly not one of these. In every kind of skilled action from the simplest to the most complex, there is some kind of apprehension of the direction in which evidence is moving, or of the varied directions in which it might move.

A simple and narrowly limited form of this is what may be called "receptor-effector anticipation." "If we make a small two-way switch of the joy-stick variety which can be operated east-west by finger and thumb, and get a number of people of comparable age—say from 17 to 28 years—to move the switch from side to side as fast as they can for three or four minutes, we shall find that their mean rate is about 12 a second. If we now arrange a display of 30 small circles, six in each of five horizontal rows, with inset pointers randomly disposed east or west, and get the same observers to read the pointer directions in succession left to right beginning with the top horizontal row, we shall find a mean time of about 19 seconds required for each complete reading."[1]

Now suppose we use the 30-circle display but require manipulation as well as reading. We might reasonably expect that some manipulation time would be added to the interpretation time, and that, since the pointers are disposed at random, the total time would be something more than the sum of the times already separately established. In fact the mean time needed for both reading and manipulation, for any such bank of 30 circles, is of the order of 19½ seconds. Something is happening when interpretation and manipulation are combined which materially speeds up the former or the latter, or both. In fact, it is easy to show experimentally that in such instances interpretation runs ahead of manipulation, and the appropriate movements are facilitated when they become due. Neither the items of evidence, nor the movements are treated merely as a succession: both become series, the members of which are linked, possess an order, and an elementary form of direction.

We can say for certain that when items of evidence are presented one after another for sensorial observation, and each of them as it occurs has to set up an appropriate movement, a

[1] *Anticipation in Human Performance*, pp. 3–4.

basic anticipatory function comes into play. The observation runs a little ahead of the point which action has reached; and each item of evidence is responded to as having a limited directional property.

The range, or span, of receptor-effector anticipation, however, is extremely narrow. Operations at this level gain by a display of two, or sometimes three, items simultaneously, but little, if at all, beyond that. Yet in innumerable skilled activities of daily life it is certain that nearly everybody surpasses these limits of range. How is this done?

One way—probably the most familiar—is by the grouping of signal elements. "Groups of stimuli, presented at the maximum rate for discrete stimuli"[1] can be dealt with fully and adequately. What has happened is that chunks, or parcels of stimulus items have achieved the same functions that the original more isolated sensory element possessed. This introduces a new feature into the anticipation picture and means that some novel processes are taking a part in the identification of direction.

E. C. Poulton has called this new stage one of "perceptual anticipation." For details about this reference must be made to his interesting and important original papers.[2] The essential basis of the development is that the variety and *tempo* of natural events are often such that a simple anticipatory process resting

[1] M. A. Vince: *Rapid Response Sequences and the Psychological Refractory Period*, Brit. J. Psychol., XL, 23.

[2] E. C. Poulton: *Eye-Hand Span in Simple Serial Tasks*, J. Exp. Psychol., 1954, **47**, 403–10.

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E. C. Poulton and R. L. Gregory: *Blinking During Visual Tracking*, Quart. J. Exp. Psychol., 1952, **4**, 57–65.

H. Kay and E. C. Poulton: *Anticipation in Memorizing*, Brit. J. Psychol., 1951, **42**, 34–41.

upon the inherent timing differences of single item stimulus-response behaviour cannot satisfy the practical demands. The weight of emphasis is now shifted towards the structured character of grouped stimuli and events, and it is this structured character which can be used to project action towards a phase of behaviour yet to come.

Broadly there are two ways in which this is done. One is when the situation which is going to require response can be observed before the moment for appropriate action has arrived. Poulton has named this the "preview" case. What is perceived during preview is stored, and used later by immediate or remote recall. The other is the case in which some characteristic relationship of "dominant"[1] items in the perceived structure—like regularity, or symmetry, or grouping of numerical detail—is used as a principle, or a rule, to direct subsequent action.

In both cases the action, when it occurs, is made smoother and quicker, and, as compared with the receptor-effector case, the range, or span, over which performance can be affected, is much enlarged. In the preview instance the limit is set by that number of related items of display which can be dealt with in the time available for the original observation, and by whatever it is that happens to these items during the interval before action is called for. In the case in which some rule of arrangement is applied, there is theoretically no limit of range; but in fact the principle can continue to be used until some contradictory empirical evidence from the subsequent environment breaks in and stops the rule from operating any further.

If I am right, perceptual anticipation comes into the picture mainly because the behaviourally more basic receptor-effector anticipation very often breaks down. A vitally important result, however, is that potentially the directional properties can come to be appreciated as belonging to the *evidence*, and not merely as indissolubly bound up with bodily movement.

When the evidence that is available is presented "all at once," as in a static display, within considerable limits of time and complexity we know what features and what relations are most likely to be perceived, retained for as long as is needed, and used to aid subsequent action. But the greatest number of

[1] See *Remembering*, pp. 31–2; 80–81, 209–12.

use to him, will be dealing with the signals of the moment, displays that are important for manipulatory and other bodily skills are presented in a succession. None of these can be treated as a line of discrete items. They possess a structure, number, and order laid out in time; and we know a great deal less about what perceiving and recalling can fundamentally achieve when they have to deal with interrelated stimuli and events in sequence, particularly when the displays and the motor response to the displays have to overlap. On what evidence there is, it seems extremely likely that when such overlap occurs the structure and order perceived may be subject to extremely rapid initial memory decay.

Perceptual anticipation marks a great step forward, particularly because it makes possible the discovery that objective evidence has properties of its own which are not completely tied up with that specific environment in which at a given moment they are found. But it still has fairly strict limits of time, of the complexity of display with which it can cope, and it is very much at the mercy of the chance happenings of the moment.

Once more it is clear that a large number of the skilled activities of daily life have characteristics which could not possibly be accounted for in terms of perceptual anticipation. Here I think we may get the clue that we need from some very illuminating experiments carried out by Dr. N. H. Mackworth.[1] The situation considered is one which will be familiar to anybody who knows something about contemporary devices for traffic control. An operator has charge of a number of mobile units which must be brought to required places at required times. In the experiments the units are operated, as indicated by a succession of signals, in a predetermined random order. All the time, within the limits of his receptor-effector time-lag for the type of signal used, the operator is dealing with current signals, and at the same time preparing to deal with signals that still lie ahead. It is possible to use a variety of "underlining" devices in order to emphasize any special importance which signals not yet reached may have. The operator then, if the sort of pre-knowledge which such signals can convey is of any

[1] N. H. and J. F. Mackworth: *Op. cit.*

and also preparing to decide how to deal with units that lie a number of steps ahead in the series of developing events.

What any kind of "underlining" does in such cases is to give foreknowledge of when or where, at a point not yet reached, some important move will have to be made. It is something different from the "preview," for it is not using what has occurred already to settle the detail of action that must be performed soon; but using information about what will occur to prepare for appropriate action when the time comes.

Also it has to be distinguished from the perceptual response to structural features such as regularity, symmetry, and numerical grouping. The structure that it *is* concerned with is not principally that of regularities or repetition in the display, but rather that of the structure of the skilled response itself: at some place ahead something important is going to happen, and a key move will have to be made. This move, however, may have a wide range of freedom, and the foreknowledge that is given and is effective, does not closely specify precisely which of a number of possible things will have to be done. Thus the most important feature in these experiments is that in this case there is no necessity for any direct discrimination of the sensory attributes and qualities of an on-coming item of display, or of dominant relational characteristics in a particular display structure. All that is required is an indication that at some fairly well-defined stage in a developing display sequence, there will come an item which it will be especially important to treat in a way appropriate to the whole operation when the right moment for action is reached. This indication can always be conveyed in any one of a number of ways, and whichever one is chosen, it is symbolic in function. In Mackworth's experiment the items concerned were floodlit, but any device whatever which would give the items the required priority would have done just as well. We have in fact reached that phase of anticipatory response in which symbols replace or supplement direct sensory or perceptual response, and it is this, more than anything else, which gives to "foreknowledge" its greatly increased possible range, both in space and in time.

Every rational person has already made or learned the discovery that symbols can have this general anticipatory function

by the time he reaches a stage at which he is able to think in the specific sense of this discussion. Most people probably learn to use this kind of forward reference, because they are members of a social group in which the device of general warning signals is very common. For example, most road signs operate in just the same way as the flood-lighting used in Dr. Mackworth's experiment. They indicate that at some limited distance ahead, an instance of a class will be met with, and special behaviour may be called for. The instance may be more or less defined, and the behaviour demanded will depend also upon incoming perceived information as the critical point or region is approached, and because it is conditioned in these ways, it will remain of the "this, or this, or this . . ." type until some moment of final decision is reached.

These are the three fundamental processes which enter into the identification of direction. Receptor-effector anticipation comes into play at a very much simpler stage of development than that with which we are specially concerned, but it contributes that important part of the whole directional response which means that the information of the moment is treated as a cue for an immediate response, *and also* for a next to an immediate response. Perceptual anticipation, too, is established before thinking proper becomes possible. By using structure and features of structure as rules related to, and perhaps determining, subsequent action, it makes it possible to treat direction as an objective character of evidence. When we discover or learn how to use warning signals, or symbols, to indicate that "this or this or this . . ." may have to be done when some region not yet reached is gained, we have that kind of direction spotting which is most characteristic of thinking.

(*b*) *Making Use of Directional Properties*

Nobody is entitled to assume that the appreciation that evidence is proceeding somewhere, and that the utilization of the evidence must possess direction, are by themselves sufficient to ensure that the evidence will, in fact, be used. There are plenty of cases, especially at a relatively elemental level of closed system response, in which it may seem that this *does*

happen. For example, a person given "2, 4, 6," may himself proceed immediately "8, 10, 12 . . ."; but sooner or later he will stop, and the stop will be because a terminal point is provided by the evidence, or because something more interesting breaks in from the environment, or because the interest that prompts his continuation of the series is satisfied. In all of these instances we have to admit that an active agency of some kind must be at work prompting an advance by steps to a terminal point, which is either supplied from outside or itself determined by the activating interest.

With closed systems of simple structure, containing just those repetitive, regular, or symmetrical elements that facilitate perceptual anticipation, the interests commonly have a formal character, in the sense that within any social group there is great agreement in the use made of the evidence. Anybody who cares to try the experiments, however, will quickly realize that there must be some active process at work over and above the appreciation that information supplied can be treated as having direction. There will always, as I have said, be the odd person who will take the conventional evidence and carry it to an unconventional conclusion; and there will be others who will say, "Oh, we can't do anything with this." But when they are pressed it soon appears that they can do something, and also that what they do is very often precisely what others have done without pressure. What they should have said is not "We *can't* do anything with this evidence," but "We are *not prepared* to do anything with this evidence; it is not interesting enough." The pressure brings some effective interest into play but adds nothing to the evidence.[1]

In the case of all those closed systems in which—as long as the operations are restricted to members of a social group whose boundaries can be fairly definitely stated—the gaps are filled up in a highly uniform manner, there must be a very close relation between the effective interest and pre-established social conventions. That is to say, what the convention expresses, and what the interest realizes are both settled by the structure of the system concerned, but that the interest goes to work to

[1] It may be worth comparing this section with what is said about interests in *Remembering*, pp. 208–14.

fill up the gaps by interlinked steps, is due directly to the compulsion of established conventions. We found greater freedom with extrapolative than with interpolative thinking, and on the whole, greater freedom still with "evidence in disguise." In all three types we also found greater freedom with words than with the other symbols, or signs, used. Two characteristics seem to go with this. One is that the actual structure of the systems, as this is revealed in information given, often becomes less and less regular, repetitive, and symmetrical, as the number of possible elements and groupings of elements increase, and the elements themselves more inherently ambiguous. The other is that the effective interests differ more and more from operator to operator even within the limits of what we call the "same" social group.

(*c*) *Selecting and Arranging Evidence*

The main preparatory work making it possible to spot and use directional properties of information is completed before thinking, in our sense, becomes possible. All the essential responses which these require are found in lower level bodily skills. Thinking takes them over and exercises them, not with overt bodily movements, but with signs and symbols. It is convenient to postpone consideration of what lies behind this switch from direct bodily action response to delayed, or remote, action responses or even to responses which never reach an issue in overt bodily movement at all. These matters can be better discussed after we have assembled some of the results of types of thinking which break out of the limits of closed systems.

When we pass to questions about how thinking selects, and especially how it arranges the evidence which leads to its terminal points, it seems as if we ought to have problems in part, at least, much more specific and peculiar to its own high level. Fortunately, there is now no need to demonstrate the basic selective character of all perception. This is everywhere accepted, and a great amount also is fairly established about both the character and the limits of perceptual selection. The selection from available information required in any thinking process can, and no doubt does, build upon what perception has

already achieved in this way. But it has characters of its own, and these are mostly connected with the fact that the items or groups of items selected have also to be arranged, or to be regarded as capable of being arranged, in a sequence leading with greater and greater necessity, greater and greater certainty, to some defined culmination.

In such arrangement of evidence a leading part is played, as the experiments indicated,[1] by the detection of points of agreement. It is well known that points of agreement are inherently less easy to detect than points of difference.[2] Perhaps the most important distinction between the two, from our present viewpoint, is that the detection of differences alone leads nowhere in particular in a positive sense, but the detection of agreement may. Thus, if one instance is observed to differ in some way from another, it may perhaps be said that something already established about the one is *not* applicable to the other, but if two instances are observed to agree in some way, it may possibly be implied that something already established about the one *is* applicable to the other. It seems fairly certain that, in a cognitive sense, all advance of knowledge comes by using agreements to get a move on, so to speak, and then using differences to keep the move within limits, and to show where a new direction of move becomes necessary.

Differences stick out in untutored perception. But it is also true that perception normally has a content of practical achievement, and under the influence of this, perception of points and features of agreement becomes well established. In thinking these fundamental perceptual responses are taken over and used in special ways. Items and groups of items making up the available evidence are specifically explored for features of agreement and difference. Then those items and groups are picked out which display the greatest number of, or the most important, points of agreement, and these are arranged in a sequence such that each successive step becomes empirically more probable than the preceding step. As thinking becomes more complex, that is, covers a wider temporal range and descriptive variability of evidence, the criterion of importance tends to overweight that of number. Here "importance" means

[1] Cf. pp. 45–7; 59–60, [2] Cf. *Mind at Work and Play*, pp. 63–5, 115–19.

those features in evidence which, remaining constant, can be applied in a sequence so that a terminal point becomes more and more clear and more and more uniformly and definitely determined as the sequence proceeds.

All this should make it plain that the selection and arrangement of evidence are inseparably bound up with the responses to directional properties already discussed. To speak as if the thinker picks out and disposes his facts by some kind of entirely intellectual comparisons, generalizations, and abstractions, is a mistake.

All the way through this book I have taken the view that thinking is possible only if much that precedes it in an order of development continues to exercise an influence. This is true both in that the thinker is able to avail himself of evidence no longer a part of his immediate external environment, and in that the ways in which he treats the evidence very often have conventional characters which could not possibly be constructed on the spur of the moment. Perhaps the most critical of all thinking problems have to do with what sort of processes of storage of evidence thinking demands, and what are the implications of the uses thinking can make of the stored evidence.

3. STORAGE[1] AND CLOSED SYSTEM THINKING

Since thinking is a high-level response, whenever it occurs it must be able to draw upon information already acquired and to make use of responses already established. Consequently, any theoretical consideration of thinking must deal with problems of the storage of information and its recall.

The experimental evidence collected shows that in closed system thinking there are two broadly different problems about

[1] It is perhaps unnecessary for me to say I have used the word "storage" in this section, elsewhere in this chapter, and indeed throughout the book, only in its broad and long-established psychological sense of a name for some sort of process by virtue of which it becomes possible to make use of information which was received at a time distinguishably earlier than that at which it is being used. There is no implication that "storage" in this sense is the same as the "storage" of which the computer designer speaks when what he is actually concerned with is some more or less permanent recording of numbers. See, e.g., A. M. Uttley: *Memory in the Nervous System and Storage in Computers*, Bull. of the Brit. Psych. Soc. No. 29, May, 1956, p. 28.

the storage of information. One concerns the environmental information which starts thinking going, and the other the further evidence which may have to be used to bring the thinking to an issue.

The first may occur whenever action is demanded by current environmental events which are changing in an interconnected manner, or whenever the performer himself has to be guided by external evidence, but is altering his position and point of view in the environment. If the action demanded can be met by immediate and open bodily adjustment, the whole performance is at a level of bodily skill. As such, however, it already possesses a property which all skilled response retains, no matter how complicated it may become, or to what high level it may climb. 'Skill, whether bodily or mental, has from the beginning the character of being in touch with demands which come from the outside world"[1] or, in a wider sense, from evidence that is treated as objective.

Thus, whenever evidence from outside is to be used within narrow time limits, either to initiate a series of skilled movements or to start off a train of thought, the overriding immediate demand is for complete loyalty to that evidence. This, of course, does not, and cannot mean that absolutely all the detail that is present must be assimilated, but only that the unavoidable gaps have to be such that they can be filled—by direct action in the case of skill, and by some kind of symbolism in the case of thinking—in accordance with the items that are assimilated. When this does not happen, skill breaks down into the sort of behaviour we call "awkward," and it may reach some unwanted termination; or the thinking at most satisfies only some of the demands of the situation. The experiments, even in the relatively definite case of the closed system, have shown that, so far as initial evidence goes, the most common feature of unsatisfactory thinking is that detail which is required to give the information its full directional properties is overlooked or forgotten. We may expect that such maltreatment of immediate evidence will become more and more important as the thinker moves into freer fields.

[1] See *The Measurement of Human Skill*, Brit. Med. Journ., June 14, 1947, pp. 835–8; June 21, 1947, pp. 877–80.

Such considerations appear to indicate that extremely short-term storage, or, more exactly "immediate recall," must be able to preserve exactly and precisely all the concreteness of original observation: something goes into store and almost at once can come out again exactly as it went in. It may be so. Unfortunately, it is impossible to be certain on psychological evidence alone. It is equally consistent with the behaviour, whether of bodily skill or of thought, that the original evidence does not go into store at all, but remains active throughout the whole of what may be called the "short term" or "immediate recall" interval. The familiar recorded phenomena of the "delayed recall" experiments could just as well fit one view as the other. All that can be said, apart from an exploration of possible neuro-physiological approaches, is that if the "immediate recall" which must be used to keep up with current events, is held to require storage, then nothing can happen to the stored evidence except possibly the loss of some of its detail.

When we look back over the experimental evidence so far collected, we cannot fail to notice that if information of earlier date is used to supplement and complete the initial evidence, far more can happen to it, since it was first "stored," than the simple omission of detail. Before we consider these differences further, we must look at another matter.

When evidence issues at once in bodily skill, or starts up a thinking process, always the original observation is fragmentary and possesses gaps. We have seen repeatedly that even in the simplest and most constrained thinking the same terminal point can be reached in more than one way. This repeats at a higher level what has already been established in the case of skilled bodily performance. "The same stroke" in any ball game can demonstrably be achieved now in one way and now in another. Thus information which is varied so far as its input gaps go, commonly produces effects which can be regarded as identical so far as their terminal points are concerned. Moreover, it is as certain as anything psychological can be, that such identity of termination with variability of approach is not normally achieved through any process of representation, copying, or anything of that sort, which is added to the observational processes that receive and interpret the original evidence.

Intelligence received with varying gaps can directly produce movements of body and of mind having what descriptively is a common terminus. There is, in fact, a kind of native and inherent generalization, which is not, however, achieved by taking elements that are treated as common out of complex situations which are regarded as different. That is an achievement which certainly becomes possible at some stage or other of mental progress, but that it should be an original generalizing process seems wildly unlikely.

We must go back to "long-term storage." There is no way of defining this except the experimental one, so that all we can say is: "There is long-term storage whenever a thinking process which has been initiated by contemporary evidence must stop unless some other source of information can be drawn upon which lies outside both this contemporary evidence and the immediate external environment." Thus "long term" may mean anything from a second or less, to not far short of the full span of individual life. There can be no fixed interval of measured time, after which long-term storage begins, for in any given instance the actual interval depends in part on the nature and complexity of the evidence that must be dealt with.

Once information goes into "long-term store," subject to the changes which it may subsequently undergo, it may be drawn upon to meet a lot of different requirements. It may be remembered; made the basis and justification of belief, and of doubt; it may be utilized in a great variety of learning acquirements; it may be called upon to bolster up prejudice and bias; it may issue in thinking. Each one of these distinguishable psychological processes has to be able to use the stored information in ways peculiar to itself, and at least in part different from those of the others. We have seen that it would be consistent with psychological phenomena to suppose that "short term" and "long term" storage could be so fundamentally different as to have, in fact, different functional bases (and perhaps loci), though the performance phenomena alone cannot prove this. But there is no reason in behaviour for supposing that remembering, believing, learning, showing prejudice, thinking, and any of the other distinguishable high-level mental performances have any but a common "long term" information store. What, more

than anything else makes this possible, is that the evidence that goes into long-term storage thereafter can undergo all those kinds of change which were the main topic of discussion in my *Remembering* book. So when it is drawn upon, it can be reconstructed in all sorts of ways to meet the demands of the current situation.

If there is remembering, the information that is used always retains both a temporal and a personal reference. It is true that items and events can change the temporal order which they had when they went into storage. Something that happened long ago can be remembered next to something that occurred recently, and they can be treated as belonging to the same parcel of events for the purposes of recall. But both of them, and everything else that may go with them, still possess a more or less specified temporal mark. They all "belong to the past." Moreover, although this past need not be definitely identified as being that of the person remembering, in so far as the items are being recalled, the implication is that they have at some time, and in some form, fallen within his experience.

In closed-system thinking, the time constraint is cast off, and the personal reference has no important significance. The order of steps, for example, with which we have been much concerned, is an order of implication, or of inherent necessity, and not anything to do with a temporal order of original occurrence.

It seems, then, that thinking must have both immediate evidence which is treated as incomplete, and also more remote evidence which is used to help to fill up the gaps.

So far as the immediate evidence goes, the performance phenomena by themselves do not enable us to determine whether it is subject to very short-term storage, or whether there is for all types and conditions of the reception of information, a variable, brief period, during which the evidence must be regarded as remaining continuously active. No doubt the general psychological bias is in favour of short-term storage. Words such as "immediate recall," and the view that all those common psychological experiments which require repetition immediately after original presentation are properly included in memory studies, appear to indicate that more often than not

it is supposed that the immediate and the remote evidence must be treated as subject to the same conditions, and are different only in the total time over which the conditions operate. However this may be, it does seem that the claim of thinking (and indeed of all forms of skill) to be objectively appropriate, demands very great and literal loyalty to the immediate evidence, while equally its claims to be adaptable demand a capacity to treat the remote evidence as highly fluid in its characteristics.

4. GENERALIZATION AND TRANSFER

The evidence which has been set out in the preceding chapters shows that thinking can always claim to possess a significance which runs beyond the particular items and instances with which the thinker may be concerned at any moment. Something of this kind is involved whenever data are treated as possessing direction, and it would be consistent with the results which I have collected if response to direction were regarded as at least one of the fundamental processes in the development of a generalizing function, though it does not, every time it happens, achieve generalization. At some moment, for example, in the flight of the cricket ball from the bowler's hand to the batsman, the latter's stroke must be regarded as predetermined, and he is moving into position to make this stroke before its precise instant of action is reached. Yet it can still be a specific stroke, to a specific stimulus sequence, and nothing at all need be formulated, from the beginning to the end of the incident. Moreover, at this level, it is demonstrable that the stimulus sequence can vary, within limits, from occasion to occasion, while the culminating performance remains, for practical purposes, the same; or the final stroke itself can change, within limits, though the stimulating sequence is as nearly as possible exactly repeated. There is, in fact, very much the same limited generalized performance inherent in directional response as Pavlov showed to be involved in conditioning. In neither case is the "generalization" that is exhibited in the performance achieved by any articulated process of analysis.

All this, however, is at most only the beginning of the story

of generalization as it functions in thinking, where we are operating not with bodily movements alone, but with symbols. Over and over again it has appeared that in thinking within the closed system there are two basic processes. One is the response to direction, and the other is the use of rules and conventions. Both of these mean that the thinker is moving beyond his contemporary information, but in the response to direction, he may be moving only within the instance with which he is at the time concerned, whereas in the use of rules he is moving, so to speak, from this one instance to possible other instances.

In all of the illustrations that have been given—and the same is true of the many others which I have collected—the steps in sequence which the thinker achieves are closely comparable from start to finish, or proceed by some lawful function; but the items which are linked by the steps differ from stage to stage. The rules which express these comparable or regularly varying steps are extracted from the limited range of particular data given, but they are never treated as fastened to these alone.

The greater range of the rule can be seen in two ways. First, all instances of thinking in the closed system, in so far as they proceed efficiently, achieve a uniformity of number and order of steps, once a determinable amount and range of evidence has become available, and this gives to them a significance running over from one case to another. The thinker is sooner or later compelled to take the route that he does. There is, so to speak, no element of whim in his choice.

Further than this, whenever the rule that is to be used is formulated, it can be shown to have to do with the alleged structure of the system. In some cases[1] a definite search is instituted to detect the maximum number of points of similarity between given items in the available information. So far as the thinker is concerned, the search is fully satisfied only when those points of agreement which are consistent with observed differences have been identified in such a manner that the number and order of steps in the thought sequence become the "same for all." This implies that the agreements identified and used are treated as belonging to systematic structure and can be just as well picked out from any other specific illustration as from

[1] See especially, e.g., Section 5, Ch. III.

the one out of which they have in fact been taken. The rule, then, which has been extracted from one or two instances, is forthwith applicable to a large number of others. Or, again, the *method* of rule extraction which has been effective in the one or two instances, is at once treated as extensible to many other cases.

It is most important to realize that our results afford no justification whatsoever for what is still probably the most widely accepted view about how generalization becomes possible. This is that the accumulation of instances in the experience of every individual, automatically separates out likenesses from differences. It is then found that the likenesses can be given common names, and called "properties" and "qualities," and "modes of action" whereas the differences require running descriptions, phrases, and the like, and individualize the instances.

It goes heavily against this view that to untutored observation differences make far more immediate impression than likenesses. Once likenesses have been discovered and named, or described in structural terms, their names and descriptions can be taken over and applied conventionally by anybody, even though he has not, by search, himself discovered them. Very possibly we may find that this process of using already generalized language in a conventional manner becomes more and more prominent as we move towards the thinking of everyday life. Whether interlinking likenesses are, by definite analysis, separated out from individualizing differences, or whether they and their use are merely accepted without search as conventions, nothing is happening to indicate any automatic and spontaneous "stamping in" of the former, and "stamping out" of the latter.

It seems, then, that two essentially different bases for generalizations must be recognized. The first can occur at any level of mental functioning. Whenever there are directional perception and performance, an elementary and limited range of generalization has become possible, and the direction taken can already be extended beyond the items in which it was originally illustrated. The second occurs at a higher level. When, by search, points of likeness have been discovered between items and steps of information that are available, a

much wider range of generalization becomes possible. Such points of likeness are closely linked up, in a developmental sense, with those regularities and other structural features for which, as we have seen already, there is a perceptual foundation. But being treated in this structural manner, they at once become applicable to a very wide range of different settings.

Generalization and transfer of the results of practice and training are almost always regarded as two aspects of the same kind of mental process. If the former is not to be developed in any passive and automatic manner, but requires active observation and active search, we should expect that there is no way by which transfer of practice training can be guaranteed merely by overlap of points of likeness.

Several times already, in describing experimental results, I have had to raise transfer problems. In interpolation there was exceedingly little transfer from number series to similarly constituted' verbal series.[1] In extrapolation, transfer was more likely from one instance to another if the medium employed in each instance was the same, even though the rule required was a different one, than it was when the rule remained constant and the medium was altered.[2] In the case of penetrating a disguise, it seemed that positive transfer could occur only when both the disguised and the undisguised form of the situation presented were actively explored for their structure, and an appropriate procedure adopted,[3] which took due account of the features special to each. So far, therefore, the strong indication is that transfer, like high-level generalization, is not in the least likely to occur, in any of these cases, unless there is active exploration of the situation that offers it an opportunity. More than this it seems that the exploration must definitely aim to make use of the structural features of the situation. The assumption that transfer of the results of training and practice can be effected merely by bringing together appropriate instances with like constituent items, is incorrect.

Although generalization and transfer of training and practice are related in the sense that the latter cannot occur unless the former has already in some way been achieved, it often happens that established generalizations, especially if they have stiffened

[1] See p. 29. [2] See p. 40. [3] See pp. 59–60.

into conventions, are the strongest of all the blocks that can prevent positive transfer. This is especially the case where the generalization has to do with some widely accepted way in which particular media (like names, numbers, colours, and the like) are usually handled. However, all these questions about generalization and transfer will crop up again later.

5. AWAY FROM THE CLOSED SYSTEM

Thinking, as a mental process, likes, so to speak, to go on in closed systems. For this gives it a wide apparent range, and especially rids it, as completely as possible, of all ultimate uncertainty. And there is something in thinking which is sympathetic to the uniform and the universal and antagonistic to uncertainty. But the thinker is more than a thinking machine. So there grows up a tremendous struggle between those forces which try to reduce all forms of human knowledge to the closed-system variety, and, as many of my results have suggested, all closed systems to the relatively easy and fixed interpolation kind; and those forces which lie behind the human zest for adventure and are continually revolting against and breaking out of the closed system. It is time to turn to some study of what can, perhaps, be called "adventurous thinking."

CHAPTER SIX

Adventurous Thinking—1

TWO TRANSITIONAL EXPERIMENTS

1. TOWARDS MORE FREEDOM

The broad objectives of thinking remain very nearly the same, in whatever field the thinker operates, and with whatever kind of evidence he is concerned. Always he must try to use the information that is available to him so as to reach a terminus, based upon that information, but not identical with it, and he must so set out, or be prepared to try to set out, the stages through which he passes, that he can reasonably hope that where, for the time being, he comes to rest, everybody else who is not mentally defective, or mentally ill, or abnormally prejudiced, must come to rest also. In some instances, as we shall see later, the thinker's hope is that others will approve of, rather than be compelled to reach, the result which he eventually attains, and at which he stops. As thinking moves away from those closed systems which have been our main concern up to now, towards more open and less bounded regions, although its strategy changes little—except perhaps in the one point that it may seek approval rather than a literal confirmation—its tactics may have to be varied in a number of ways. Some of these I shall now try to consider and illustrate.

We have seen already that there are two ways in which experiment is usually demanded even when thinking takes place within a closed system. The occasion to think is given when information available is treated as providing an opportunity to move on somewhere else; but it is a very rare thing indeed that when anybody begins to think, only one direction of move is possible. If we are interested in trying to understand the process, we shall wish to find out what are the odds that any

thinker—if he moves at all—will move in this, that, or the other direction. To discover this we usually have to resort to experiments of the kind which I have already discussed.

There is also the other sort of experiment which the thinker himself must often try, even when he is working within a closed system. He is faced with a number of possible directions of move, and when he starts off along one of these directions he is, in a way, conducting an experiment. Suppose that from stage to stage along his chosen line of advance, he appears to be opening up a larger and larger number of possible directions of move; or suppose that it comes to appear to him that if he keeps on without a change of route he will miss his supplied, or anticipated, objective, sooner or later the thinker will give up, or experiment with some other direction.

Thus even the relatively simple closed system case requires experiment for its understanding, and it usually requires another kind of experiment for its prosecution. A critical consideration of experiment as the method more than any others important for empirical scientific research, introduces some new features, however. The scientific investigator very often appears to be working within a system which cannot be treated as already fully structured. Often he plans his experiments, and uses them, in order to discover structural characteristics which, without his experiments, he could not know at all. He may, especially in biological fields, be concerned with structured systems that are still in course of development. His aim, then, is not simply to extend and define knowledge within a system that is to be treated as already completed, but to help to develop and understand systems which are so far literally incomplete.

As experimental science has gained wider and wider fields, and won increasing recognition, it has often happened that critical stages for advance are reached when what has been called one body of knowledge can be brought into close and effective relationship with what has been treated as a different, and a largely or wholly independent, scientific discipline. Whenever that happens the experimentalists who are at work in any of the regions concerned, move, so to speak, forth and back across what may still in various ways remain boundary lines marking out different fields of study, and as they do this

the items with which they operate, on either side of these lines, may be found to possess (or to have acquired) properties which nobody could have attributed to them before.[1]

Considerations of this sort show that the psychology of thinking in the case of the experimental scientist, must face some new problems. In broad effect, the starting, and near-starting, moves that are theoretically open to the experimentalist are often less restricted, and decisions as to which of them is most worth early exploration are more empirically determined. Moreover, the criteria by which prospective success is assessed at stages of exploration short of the final one are different. Just as much as the thinker in the closed system, the experimental scientist is in search of a terminal point from which no rational person can escape, but it seems as if he is also seeking much more than that, though what and how can become plain only when we have some instances before us to study.

2. TRANSITIONAL EXPERIMENTS

There are many situations which possess some of the characteristics of closed systems, as we have considered them, and also some of those that belong to scientific experiments. For example, suppose, starting from a particular place, we are given an objective indicated, in a general way only, as N., S., E. or W. of the starting-point. We have a sectional map which indicates a possible choice of directions along which we may move. When we reach the boundary of the first sectional plan, we are given another sectional map, and so on in stages, until we reach the end of the journey. In some respects, obviously, all of this procedure works within a closed system. The whole road scheme is complete before we begin the projected journey. No decisions as to direction that we may make at successive stages

[1] An instance can be drawn from the study of Genetic Recombination in investigations of bacteriophage: "When two related phages, differing from one another in at least two characters, infect the same bacteria, a new phenomenon is observed. This consists of the appearance of phage types that were not present in the input and represent new combinations of the character of the parents." Luria: *General Virology* (London: Chapman and Hall, 1953, p. 189). Any experimental scientist will probably be able to identify instances within his own field of study.

on the way to the final objective can have any possible effect on the disposition of the roads in the sectional maps not yet reached. But in some other respects our position is not unlike that of the experimenter. At every decision we make, with every consequent move up to the limit of a sectional plan, fresh information, or more evidence, is made available, and although descriptively this is just the same as it would have been if we had made different decisions, its significance relative to the objective is very closely dependent upon the moves, and the extent of the moves, that we have already made.

It is true that with sectional map-reading, additional information is made available and new decisions are called for at predetermined stages, whereas in the scientific experiment additional evidence is being developed, or seems to be being developed, all the time; and decisions are being shaped, or seem to be being shaped, continuously. At the same time, it is not unlikely that the ways in which additional clues are used, and the conditions which lead up to particular decisions, remain much the same in both cases. At any rate it is worth trying to find out whether this is so.

Any of the many other possible situations in which an objective is defined in general terms to begin with and there are several possible lines of exploration, some of which yield information that may eventually pin-point the objective might similarly be shown to have some characteristics in common with scientific experimentation.

H. C. A. Dale, for example, has attempted to develop a technique which may have a direct bearing upon much of the procedure of the experimental scientist. He sets up what he calls a "searching task" which must be carried out in stages. There are two main cases: (1) "structured situations, in which all points encountered during the search yield information regarding the direction in which to proceed," and (2) "unstructured situations in which this information is unavailable."[1]

I now propose, therefore, briefly to consider (*a*) Map Section Reading; and (*b*) Searching in a Structured System as possible transitional experiments leading up to a more direct

[1] H. C. A. Dale: *Searching in a Structured System*, A.P.U. Cambridge, pp. 247–55, 1955.

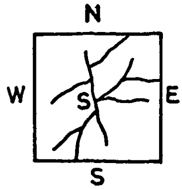
discussion of the principal characteristics of the thinking of the experimental scientist. Obviously other situations might have been selected, and I hope it will be equally obvious that each of these "transitional experiments" raises psychological problems of much interest, and that they would repay more prolonged and careful study and analysis than I can give to them here.

Probably everybody will notice immediately that there is another important respect in which both these transitional cases differ radically from scientific operations which are designed chiefly to advance knowledge, rather than to answer some specific question. They both have definite stopping-points which are inherent in their design. When these terminal stages have been reached the operation is over, and there is nothing in them to open up new avenues of exploration. When the experimental scientist completes what he would call a "fruitful" experiment this is not the case; his stopping-point is temporary only and leads beyond itself.

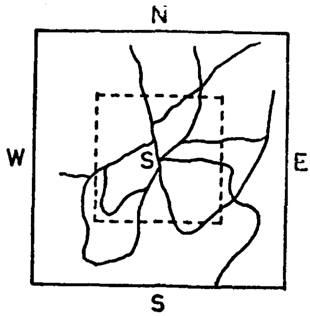
(*a*) *Some Points about Sectional Map-reading*

I cannot claim to have made anything more than a very limited beginning of a study of the possible use of sectional maps as an experimental way of opening up some of the problems of scientific thinking, and even, perhaps, of suggesting likely answers to these problems. Enough has, I hope, been done to show that this general technique, and others which could be based upon it, are worth an extended exploration. The varieties of design, of serial presentation, and of overall structure that can be used, are very numerous, and here I shall attempt to take up and illustrate only a few of the many points which could well be raised.

The basic plan can be illustrated simply. The observer, or "explorer" is given in succession a number of road section maps. Here is a relatively straightforward instance.



No. I



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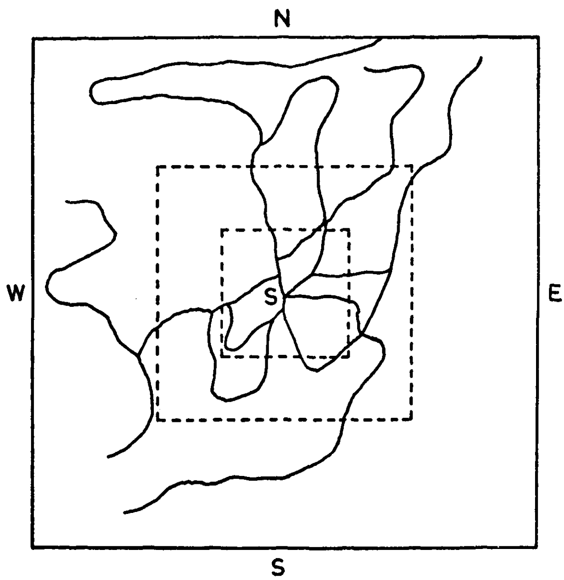
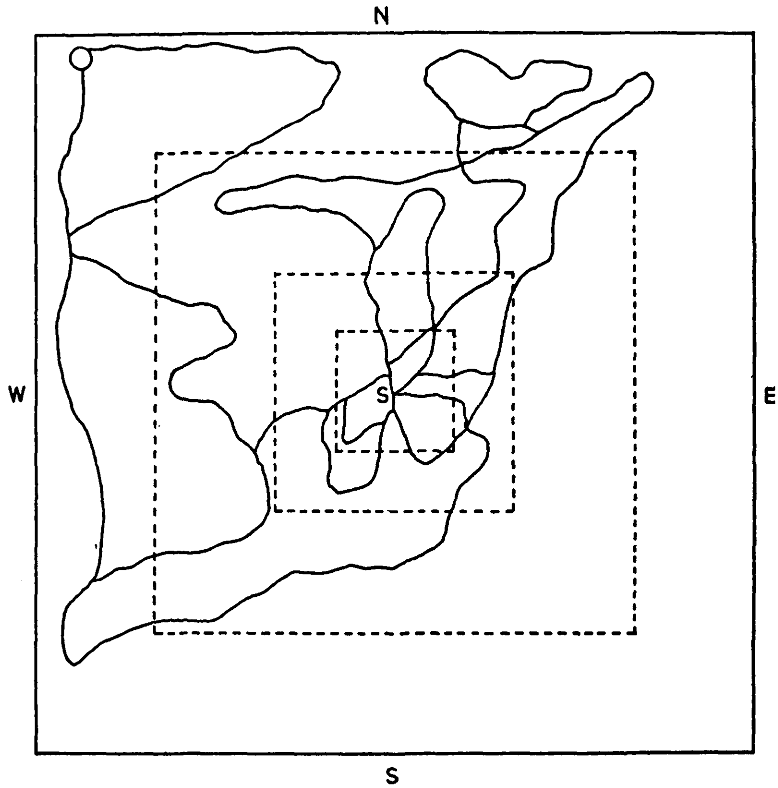


No. III



No. IV

Instructions, given with the first section plan are:

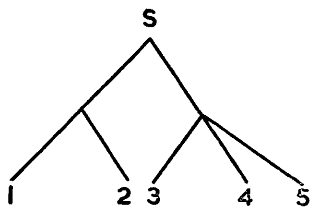
"You set out from the point marked S and your aim is to get to a spot somewhere to the N.W. Choose which road to start on, and when you have got as far as you can on this plan you will be given another sectional map, and so on until you get to the final map on which the place you want to reach will be marked O. At any stage you can, if you wish, go back to the starting-point or to some position short of the starting-point."

The observer ("explorer") may be given the information on the basis of which he chooses his route in a number of different ways. Each successive section can, for instance, be superimposed upon what goes before it, so that at every stage he has the

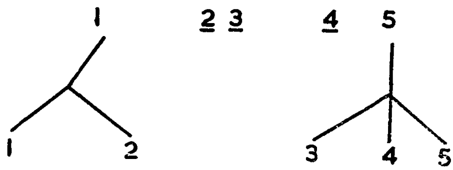
complete map up to the finish of that stage. Or he may have to remember one, some, or all of the preceding stages.

Furthermore, the overall road system may appear to have no determinable principles of structure, and particular roadways may seem to wander about much as they appear to do to the uninstructed traveller in most rural districts in this country. Or there may be a graded structuration up to the most complete regularity of lay-out.

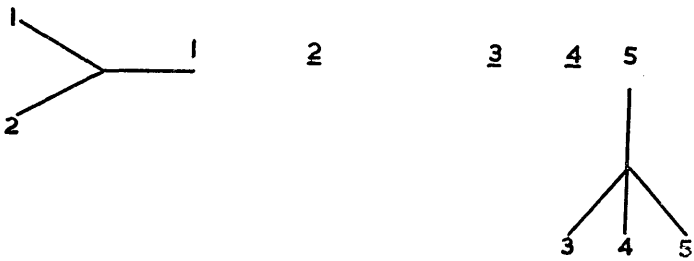
Again, there may be no dead-ends, as in the illustration given, and all roads out from S may eventually reach the objective, though along routes of varying length. Or there may be dead ends, so that there are bound to be return journeys unless an open route has been selected. An instance combining regularity of lay-out with dead-ends might be



No. I



No. II



No. III

(The objective is given as N. of S.)

It will be seen that, provided the whole lay-out is going on in this regular way, each "stage" or section has two ways out, one of which gives a choice of two roads and the other of three; that always roadways marked 2, 3, and 4 are dead-ends, and that of the remaining two routes, road one will give a two-choice, and the other a three-choice situation; that at some section stage (here marked III) either the two-choice or the three-choice situation will make a change of direction towards the objective defined in general terms at the beginning of the experiment.

All that I propose to attempt to do at present is to indicate, without detailed analysis, some of the suggestions which emerge from these sectional map-reading experiments, in so far as they may help to provide an orientation to more direct studies of scientific thinking.

Firstly, there is the persistent question of how and when response to direction takes charge of thought activities. Obviously in these map-reading instances "direction" is to be taken in a literal spatial sense. The map-reader moves, in imagination, along a given line of route and can estimate at once whether when he comes to the edge of his section he is likely to be nearer to or farther from his general objective. The road system designed can, of course, for this kind of "direction," have whatever characters the experimenter selects. Initially no route may lead towards the objective (as in the second of the two illustrations given), or all routes may lead towards the objective, or one (as in the first illustration) route may lead towards the objective, or there may be any desired intermediate case. When all, or no, routes set out towards the general end given, and there is no other noticeable characteristic in which the routes differ, early exploration gets as near random as it ever does. But it never gets entirely random, in the sense, for example, in which the sorting of lottery tickets is random. If a system which possesses no objectively detectable differential structure has in any way to be dealt with by a human being he cannot avoid treating it as if it had such a differential structure, so that as soon as ever he has anything to do with such a system it is true to say that not all of the things he might do are equally likely. Perhaps a simpler and more "natural" way of putting this would be to

say that in such instances an interest, or interests, which an observer has already formed may come into play. Then the same basic function which settles whether the observer will do anything or nothing about evidence,[1] at the same time gives an unavoidable bias to what he can do.

In sectional map exploration, direction, in the sense just described, tends to be given priority at all stages of experimental search, but most decisively in the early stages. Thus with the first illustration given it is very nearly certain that any normal map-reader will start out along the only route that runs nearly N.W., and he will keep along this same route without deviation until the third sectional plan is reached. At this point the preferred route turns directly E., and it can be seen that two other of the starting routes have joined and seem to be going more nearly in the direction required. At this stage it appears that a small majority of explorers will prefer to continue along their chosen route while the others will retrace this path part or all of the way back and set out afresh. But the later in a sectional map series such a change of apparent direction occurred, the greater was the proportion of readers who nevertheless kept their route unchanged. None of the subjects of these map-reading experiments knew ahead how many sections would complete a series, also none knew at any stage until the last one how near he was to his objective. It is, however, the case that this kind of literal directional evidence was given absolute priority in the early stages of experimental search, and then with diminishing effect as the search went on.

The fact that once a route has been consistently followed for some time a fair number of people (probably most people) either prefer to continue along it, or are averse from moving away and starting afresh, even though the evidence that induced them to select that route is now apparently contradicted, may well be worth further study. It can be said at once, however, that, at least in this variety of prospective extrapolation, a lot of people see no overwhelming virtue in economy of route. Whether or not they may say that "the best way is the shortest way," this is not a dominating principle in their action.

[1] See p. 84.

A related question is whether the number of possibilities that seem likely to be opened up by any particular move in a series of moves continues to be as important when thinking becomes freer and more adventurous as it seems to be in the fully closed system. The indication in the case of closed systems was that the strongly preferred move is one that reduces the number of probable next moves. It was in the hope of obtaining some experimental evidence about this that I used map sections in which, to begin with, either all routes, or no routes, were in the direction of the objective, but some routes consistently divided into more next possible roads than others. One very simple instance is the second of the two illustrations given above (p. 104); there are many other possible variations on the same theme.

In the case of this particular illustration, once the first step was taken any subject who was prepared to treat the road system as possessing a constant structure quickly realized that after the first step, neither direction gave him a true multiple choice. For at each succeeding stage roads 2, 3, and 4 were "dead ends." Obviously, however, it is easy enough to design sectional map systems in which multiple choice may be set against dual choice for as many stages and in whatever number may be desired. Under such conditions, so far as my own experiments have gone, no strong or consistent preference was shown for routes offering a smaller number, or a progressively diminishing number of next moves. When the map sections offered a succession of either/or alternatives, and also a succession of multiple choices, it was a little more likely that exploration would begin and continue along the latter, to return to the former only if at some stage one of the either/or routes took a decisive change of direction towards the general objective, and not always even then.

This may perhaps be a case somewhat specialized to operations like route finding on incomplete evidence. But if a generalization is legitimate from these map-section results it would seem that, when a thinker is working in an open, or relatively open, system (roads, for example, may proceed in *any* direction) he inclines to prefer the evidence which releases the greater rather than the smaller number of possibilities. I

do not wish to suggest that in adopting this kind of procedure the thinker is following any clearly formulated or definitely articulated hypothesis about probabilities. It seems certain, indeed, that he must be working according to some kind of probability criterion which, however, may remain for him hardly defined at all. In any system having little known fixity of structure, on the whole, moves which release a larger number of possible next moves will be preferred to those which release a smaller number. The *working*, though not the formulated, rule seems to be that it is better to explore along the line of the greater number of possibilities, because it is more likely that the one sought will be found when there are a lot of chances than where there are only a few. It seems extremely likely that there must be some limit to this, and that when the number of possible next openings exceeds a certain range, the fact that with very many possibilities the small probability that any single one is the one wanted will override everything else. But I have not been able to determine this limit. Anyway there seems to be little warrant for the assumption that has often been made that the human brain shows an inherent preference for dual-choice systems, or for short cut, or clear-cut, situations in general. Already, in these transitional experiments, where thinking takes place within a sort of pseudo-open system, we can see that there is something attractive to the thinker about risk and adventure. He will often move, perhaps deliberately, and perhaps without much realization of what he is doing, along a line that opens up a large number of chances, though, so far as he can know, any one of these can have little likelihood of leading him where he wants to go. Maybe what this amounts to in a psychological sense is that a larger number of chances are being treated as a bunch, and that in the lump they outweigh the attraction of the smaller number of chances. Perhaps, also, we are now moving into a sphere in which thinking becomes more of a sport or a game than it can well be in the genuine closed system.

There is an obvious objection that all sectional maps which are actually used for any practical reason will contain many more concrete, representational details than those devised for these experiments. There will be river-courses, railway-lines,

and often outstanding geographical and "cultural" features. During the war of 1939–43, for example, when sectional area maps were much used by airmen, both "natural" and "man-made" features of a landscape were often specially marked. It is not difficult to show that such descriptive features, which make use of knowledge that the explorer already possesses, are given greater and greater importance as he gets his general route more and more opened up. How specialized knowledge enters more and more into the later natural development of thinking no matter what its field, will perhaps become clearer as we go on.

(*b*) *Searching in a Structured System*

H. C. A. Dale has attempted by experiment to discover something about the principles that come into play when a person has to decide how to search for an assigned objective, and that objective is known to lie within some more or less defined system. He distinguishes between "structured" and "unstructured" systems; but this distinction is not of any great importance from our present point of view. For him a system is unstructured if it is such that search for the assigned objective can just as well begin at any stage in the system as at any other. If, under such conditions, the first attempt is successful it is so only fortuitously. If it is not successful nothing whatever has been learned about any preferred order or direction for subsequent search. There is indeed no possible objectively preferential order. But, as everybody knows, plenty of systems can be found in which particular search-points or check-points can be used to settle the most economical order and direction of subsequent search.

Dale says: "An example of an unstructured situation is that of searching for a ball in a field. A ball lying at one point in a field has no effect whatever on the rest of the field, therefore no amount of effort expended in searching one corner will give any indication of whether it is best to look next in the middle, or in the opposite corner" or anywhere else.

This is obviously correct so long as the searcher is confined to search in a limited area by action, but if he can ask questions, provided only the ball, the field, or both do not change in some

way that affects relative positions, then—in theory at any rate—there are always certain questions which, whatever the answer, will indicate the order and direction of those subsequent questions which will find the missing ball by the most economical route. In fact there is literally no "unstructured" situation unless it is one in which changes are going on at random all the time.

However, the study of systems which present a number of possible check-points and some of them give more, some less, information about how to proceed in an economical manner, is one of great interest and importance. The case that Dale takes is that of the location of a fault in an electronic circuit. Throughout the whole of these discussions I have treated thinking as a process which generally, though not inevitably, proceeds through a succession of steps or stages, and that there is some connection of necessity, or of preference, from step to step. I have all the time agreed that there is also the intuitive kind of thinking in which the steps are not articulated, and more consideration must yet be given to this kind.[1] But provisionally I have accepted the view that although in intuitive thinking the steps are not stated, they could be, without any change of issue. Dale's case, therefore, is particularly interesting in any study of experimental thinking, for "electronic circuits consist basically of chains of 'stages,' and if a signal is fed into the input, checks will show it to be correct at all points up to the faulty stage and incorrect at all points beyond it." Thus information is made available both about the direction and about the order in which to proceed.

The details of Dale's inquiry may be gathered from his paper. The general result is that, especially with naïve, although intelligent searchers, it is most likely that the method of search adopted will be an uneconomical one. Theoretically search-points ought to be chosen so as to get maximum possible information. "This is best done if the searcher proceeds by asking general questions, 'Is my objective in this half or that.' To begin a problem by asking 'Is the objective in this stage' is a very inefficient procedure." However, in Dale's main series only one of his twenty-four subjects used the theoretically best

[1] See also M. L. Cartwright, *The Mathematical Mind,* James Boyce Memorial Lecture, Oxford Univ. Press, 1955, pp. 9–12.

method. Also those who began by using a method which demanded more work than was necessary did not readily change to the better way.

In "unstructured" systems, in Dale's sense, there is no objectively best method of search. But with these also there was little sign that direct consideration of what might constitute short paths to solution had much to do with adopted methods. The subjects whom Dale considered to be the more intelligent, however, did tend to take check-points in a straightforward order of succession, and the effect of this could be to diminish the psychological load as much as possible by avoiding any difficult strain of remembering.

(*c*) *Provisional Conclusions*

These transitional experiments have three main issues, all for the present to be treated as provisional only.

As the thinker takes up his search in the more open type of system, in the early stages directional features, belonging to the structure, or to an assigned structure, of the system in which he is working, but themselves of a general character, are predominantly effective.

As the search proceeds, these features may become less independently important, and in particular, empirical characteristics which the thinker has found in the course of his search tend to have a greater weight.

At no stage does the thinker necessarily show any strong bias towards either/or situations; towards short cuts and economical lines of search; towards numerically few risks.

The suggestion perhaps is that as thinking moves towards greater freedom one thing that happens is that the thinker is less and less concerned with the likelihood of items and more and more with that of packets, or groups of items. He is less detail-ridden, more "schematic"-minded. If we should ask for the reasons why these lumping schematizing developments take place, our present answers can of course be no more than speculation. My guess is that there are two chief reasons—they are more efficient and they are a lot more fun.

CHAPTER SEVEN

Adventurous Thinking—2

THE THINKING OF THE EXPERIMENTAL SCIENTIST

1. ITS LATE DEVELOPMENT

The thinking that gives rise to scientific experiment comes relatively late in the development of man's search for knowledge. The experimentalist does more than put questions to Nature: he tries to compel answers. To do this successfully and at the same time to avoid distorting the facts, he must know much already. For the answers that he forces from Nature must be themselves "natural," that is to say, they must be able to stand up to examination by the senses. True, the fundamental analytical items of the experimentalist are very often not themselves observable by any means, but they must have such properties that their relations, actions, and reactions, produce results that can be confirmed when they are presented for observation. "It were disgraceful," wrote William Harvey in a famous passage, "did we take the reports of others on trust, and go on coining crude problems out of them, and on them hanging knotty and captious and petty disputations. Nature herself is to be addressed; and the paths she shows us are to be boldly trodden, for then, and whilst we consult our proper senses . . . shall we penetrate at length into the heart of her mystery."[1]

In various writings Harvey himself makes it clear that he did not mean to say that never should the reports of others be accepted. He was referring only to such reports as themselves

[1] I quote this from Sir Henry Dale: *An Autumn Gleaning* (London: Pergamon Press, 1954, p. 84).

"hang knotty, captious and petty disputations," upon "crude problems." He was asserting that scientific experiment must build upon facts which are discovered as "we consult our proper senses."[1]

It will be found that every first-rate scientific experimenter is also an accurate and eager observer, whether he was one of the early pioneers who first had to rebel against traditional and dogmatic doctrines which had got out of touch with facts, or whether, as in these later days, he frequently must make use of the reports of others. Every scientific journal, with whatever branch of knowledge it is concerned, provides plenty of illustrations of workers who show beyond dispute that they know a lot about the reports of others, but who nevertheless achieve no outstanding experimental research of their own. It is still true that merely to hang arguments upon other people's work is not the manner of thinking of the experimental scientist. If, as is now nearly always the case, he must know and use some of the published researches of others, he must also be able to pick those which will give him the initial direction that he needs. When we were considering the "transitional experiments" of the last chapter we found that in some of them there came a stage in which the directional significance of empirical features outweighed everything else. In formal and partly formal thinking this stage, if it is found at all, is apt to come late. We have now reached that kind of thinking in which empirical observation and knowledge are needed right from the beginning of the directed sequence through which it advances.

It is not, however, by sensory observation alone that the experimental scientist arrives at the point from which he can design and conduct his experiments. His concern is with the necessary connections of events and not merely with the description of whatever it may be to which these events occur. When natural, unaided perception is at work, those connections of events—if by a slight stretch of meaning they can be so

[1] "We ought to approve or reject all things by examination leisurely made, but if you will examine or try whether they be right or wrong, you must bring them to the test of sense, and confirm and establish them by the judgments of sense, where, if there be any feigned or not, sure it will appear." William Harvey: *De Circulatione. Exercitatio Altera*, Trans. by Geoffrey Keynes (London: Nonesuch Press, 1928, p. 178).

called—which attract the most immediate notice are differences.[1] At the same time, the differences which are immediately striking to natural observation cannot by themselves lead to experimental exploration. When, for instance, the processes of combustion and of respiration were regarded simply as different, nothing much more of biological interest could happen about them. Even when they came in some quarters to be looked upon as fundamentally the same, at first nothing much could be done about it, for lack of the necessary chemical knowledge and techniques. Precisely the same sort of thing has happened over and over again in every direction of scientific development, but perhaps most of all in the biological sciences. Immediately appearing differences can be used for classification and description, but not very much, at the outset, for explanation, or for any practical purpose. Then when some unusually acute observer begins to suspect whether the descriptive differences may not cloak some more basic functional likeness, he, and others, may be held up for a long time by lack of sufficiently analysed knowledge, methods, or technique. Moreover, when natural observation turns from difference to likeness and to overlap, which it does only with effort, the extraordinary, and never yet fully explained human predisposition towards generalization may come in, and for a long time prevent any further experimental advance.

For all of these, and no doubt for other reasons as well, the manner of thinking of the experimental scientist comes as a relatively late development.

2. APPROACH TO ITS STUDY

Probably it will appear that to approach the study of experimental thinking in the same general way in which I have attempted to approach that of more formal thinking, I ought to state a few experimental questions and get them investigated step by step in the laboratory by a number of experimental scientists. To attempt this might indeed be

[1] Cf. p. 86 and the reference there given. I have also considered the same evidential points in *The Nature and Place of Thinking in Medicine*, Brit. Med. Journ., April 11, 1953, p. 795.

exceedingly interesting and worth while, but it would take a tremendous amount of time. Moreover, unless such an approach were still further extended and organized, it could very well overlook the fact that experimental thinking frequently has a strong group, or social, character.

In fact, for a large part of my own life I have been watching experimentalists at work, sometimes on questions which I have been able to suggest, and I have myself worked mainly experimentally in the field of psychology. In a way all of this watching and working can be regarded as a sort of prolonged experiment on experimental thinking, and may go some way to justify the more limited approach which I now propose. This is to try a rather general analytical study of two topics of inquiry which have been studied experimentally for many years, and of which detailed and concrete information is available.

3. EXPERIMENTAL STUDIES OF MINUTE INFECTIVE AGENCIES: BIOGENESIS AND HETEROGENESIS[1]

The first of the topics which I propose to use to try to bring out some of the leading characteristics of experimental thinking is the study, prolonged over very many years, and still by no means completed, of the ways in which the minute infective agencies in certain animal and human diseases have been identified, some of their properties brought to light, and some of the problems of their origination and multiplication solved. The whole of this research is obviously related to a more general dispute, which has proceeded for centuries, between those who believe that life can be produced from dead matter, and those who hold that living matter comes from living matter only.

There is perhaps no difference more apparent to naïve observation than the difference between the living and the non-

[1] I must at once acknowledge my very great debt to the Huxley Memorial lecture which Sir Henry Dale delivered in 1935 at the Imperial College of Science and Technology, now, fortunately, reprinted in *An Autumn Gleaning*, pp. 1–20. The lecture, beginning with a glowing tribute to Huxley for his famous 1870 address to the British Association, proceeds to a masterly brief survey of the most important experimental studies of the nature and origin of infective agencies up to the early 1930's.

living. The folk expressions and stories of all times and all places are full of descriptions of this difference and of its alleged consequences. Within the field of the living, however, other differences must have very soon been noticed, and among them those which, supported by man's belief in his own superiority, distinguished "higher" from "lower" forms of life. Thereupon observation was again called upon to detect and describe points of likeness which could be regarded as linking higher to higher and lower to lower, and also as differentiating between the two. A fairly obvious one was that what are called lower forms were often found where there were mud, slime, water, and putrefaction. Then creatures like worms, molluscs, insects, and fish could readily be supposed to be generated immediately from mud and putrefying matter, and this was a common belief for centuries.

Some such stage had to be reached before experiments could begin. Francesa Redi[1] built upon the common observation that blow-flies congregated wherever there was rotting flesh. He put a gauze net over raw meat, and showed that the maggots which appeared as the meat putrefied, were hatched from eggs laid on the net, and not spontaneously from the meat itself. Only experiment could have demonstrated this. It was, however, a limited and specified experiment of narrow range.

The next step required instrument-aided observation. Leeuwenhoek,[2] with his remarkable single lenses, demonstrated that a great variety of minute life is to be found in stagnant water and organic infusions. A gauze net could do nothing in this case, and though experiments soon followed which were based upon boiling the media, sealing off the vessels when they were still hot, and demonstrating that minute animal forms could not then be observed to be present, this experiment still remained specialized, and at best only convincing over a limited range. Then, as microscopic and other aided technical methods of natural observation were improved, a whole new range of still lower order of living beings, the bacteria, became available for study. Already it was certain that a number of animal diseases were due to

[1] (1621–97) *Esperienze intorno alla generazione degl' insett*, 1668.
[2] (1632–1723.)

infection carried by these minute forms of life, and it began to appear very highly probable that many human diseases had a similar origin. That this would be found to be the case was then mainly a matter of prophecy, and so it was to remain, but with steadily increasing likelihood, for a long time. It is interesting that the prophecy was not built upon a very large number of cases, but rather upon the striking character of the instances studied, and upon the superior control techniques developed especially by Pasteur and Tyndall.

Sir Henry Dale points out that what nearly all the earlier experimenters did was to *exclude* one range of possibilities after another, as developing knowledge and technique brought within the scope of observation and investigation more and more minute forms of life. "It is of interest to note that the principle of biogenesis, when challenged . . . at different stages in a history now covering over two and a half centuries, has repeatedly found its support in methods devised for the exclusion of infecting organisms, by the use of screens, or filters, too fine for them to pass." Finer and finer forms of filtration were developed as tinier and tinier living infecting agencies were discovered, until filters were used which would exclude micro-organisms down to a diameter of about one two-thousandth part of a millimetre. This was early in the 1890's, and it now seemed as if bacterial infection had been established for practically all types of disease, and also as if in all instances the infecting agents must be introduced from outside into the infected body, there to reproduce themselves.

"In 1892 Ivanowski, working on the mosaic disease which infects tobacco plants, found that the juice from infected leaves could be passed through a bacteria-proof filter and still remain highly infective."[1] This was simply to report a difference. It was an isolated observation, and nothing much happened for seven more years, when Ivanowski's results were confirmed by further experiments of the same kind but more stringently controlled. A year earlier experiments of like kind had demonstrated that foot-and-mouth disease also could spread through an infective filtrate which was completely bacteria free.

It was the demonstration of the common properties in different

[1] Dale, op. cit., pp. 6–7.

infectious diseases which opened up the study of a new order of infective agencies. These were invisible to any examination by the ordinary microscope, they were not caught by any of the filters capable of retaining the smallest bacteria, and they could not be cultivated in artificial media.

From time to time in any prolonged series of experimental investigations, there always seems to come some experiment and some associated observations which can later be seen to have set the main stream of the direction of scientific research in the topic concerned, for a considerable succeeding period. Such a stage had now been reached in the study of these minute infective agents. The investigation of the viruses, as these ultra microscopic sources of numerous infectious diseases soon came to be called, attracted the notice of more experimenters and, as is of particular interest to us, these speedily began to develop and use a greater diversity of experiment. The search for still more radical exclusion methods continued and resulted in the construction of graded series of filtration membranes, some of which could be used to exclude viruses so small that they could not be detected by any existing known form of microscopic examination. But side by side with this, and in fact all the time very closely related to it (for some of the most successful experimenters in both fields belonged to the same British Medical Research Council Institute), went the development of special techniques of ultra-violet photography which enables a more or less direct study of virus particles to be made.

There is one series of incidents in the story at this stage which deserves special attention. Some virus infections had been found to be associated, in the infected cells, with much larger and readily visible structures. These, named "inclusion bodies," could be isolated for inspection. From 1904–6 experimental observations in France and Germany suggested that the viruses of some animal and insect diseases might consist of minute coccoid organisms which, being appropriately stained, became visible to ultra microscopic observation, and that the "inclusion body" might be a closely packed mass of these minute organisms. For twenty-five years little or no attention was given to these suggestions. Then confirming observations arrived by different routes, and also experiments on the direct

transmission of infection through isolated cell inclusions indicated that the earlier work and its results were along correct and potentially fruitful lines.

Now came, broadly, both a rapid improvement in methods of minute filtration and measurement, made possible in the main by the growth and application of chemical knowledge, and also of new methods of direct microscopic examination and measurement, depending mainly upon the utilization of expanding optical knowledge. Sometimes the two developments converged and it became possible to obtain measurements of the same viruses by both methods. The agreement then was so good that either the units of the virus made directly visible by ultra photography must be the infective agents, or else some other particles of the same size must be, which, however, for an unknown reason remained invisible.

There was a third direction of progress which followed the discovery of the viruses. This was the growth of viruses in artificial cultures of susceptible embryonic cells. Sometimes, as with the virus of psittacosis,[1] it was possible to show experimentally a close parallel between the abundance of the microscopically revealed particles and the virulence of the induced infection. Or, as with "louping-ill,"[2] all that could be demonstrated was an increased infective potency as the virus was multiplied in cultivation. The use of membrane filters in association with the cultivation procedure, showed that, in this case, the infective particles could not be more than about one-fiftieth of a micron in diameter.

From the end of 1934 to the present time, as everybody has the opportunity to know, an enormous amount more work has been done, though it still remains possible to debate those very broad questions, about the origin of living forms, which had particularly interested Huxley. Of the three lines of research already in development in the early 1930's those concerned with advances in ultra-microscopy, and still more those based upon artificial culture, have attracted the most active study. The more positive aspects of this prolonged experimental

[1] This is commonly called "parrot disease." It was given much attention when it was shown to be transmissible from parrots to man.

[2] A brain disease of sheep.

effort to understand the nature and operation of infective agents have stood out more effectively in the later than in the earlier stages of their study, when exclusion methods were more prominent.

My purpose here is only to attempt to pick out, from illustration, some of the leading features in the thinking of the experimental scientist. For this it is not necessary to try to follow in any detail the highly specialized advances in this field of virus study which have been made since 1935, and I am, of course, in no way qualified to do this. It appears that progress has been chiefly concerned (1) with the control, and (2) with the understanding of infectious disease.

Control received its principal encouragement in the discovery of penicillin, and in upshot has led to the possibility of successful treatment of most diseases of bacterial origin. Virus infections have proved more stubborn, and this has contributed to a turning of attention more recently towards experiments which offer some hope of aiding an understanding of the relations between infective agencies and infected tissues.

The last approach yields yet another illustration of how, in experimental science, a discovery may often be made a long time before it is so followed up that its full significance can be appreciated. Bacteriophages, transmissible agents which can infect and cause disintegration of visible bacteria, were discovered by Twort in 1915, and were given a fairly detailed investigation and named by D'Herelle in 1921. In one way and another bacteriophage study proceeded. It was, however, the discovery by Lwoff, around 1950, that cultures of uninfected bacteria can suddenly give rise to bacteriophage which can then be used to inoculate related cultures, that opened up a new range of investigations about the nature, the possible manner of transmission, and the genetical character and its relation to growth abnormalities of the active principle in bacteriophage.[1]

[1] I wish to thank Dr. E. F. Gale, F.R.S., for information concerning these later developments. Further detail is best obtained from *General Virology*, by S. E. Luria (New York: John Wiley; London: Chapman and Hall, 1953); from a lecture entitled *The Notion of Virus* delivered by Dr. Lwoff in London in 1957 and published by the Society for General Microbiology; and from *Microbial Pathogenicity*, a symposium published by the same society at the Cambridge University Press, 1955.

4. SOME GENERAL REFLEXIONS

That experiment had to be a late-comer in the case we have just looked at is clear enough, for it became possible only after much systematized, descriptive, and classificatory knowledge had already been achieved, by natural, and mostly unaided, observation. All along the line, experiments have been designed and carried out only when some common features, or functions, of the units, agents, or particles that are the sources of disease have become known, or very strongly suspected. More interesting, and connected with that same basic requirement for the thinking of the experimental scientist which makes experiment appear late, are the illustrations of discoveries which considerably predated any experimental use that was made of them. The first was the 1902 discovery of a bacteria-free source of infection in the case of mosaic disease, which lay fallow for seven years. The second was the 1904 discovery of the probable identity of certain viruses with extremely minute coccoid organisms, and the probable constitution of the "inclusion bodies." This remained largely unnoticed, and of little influence, for twenty-five years. The third was Twort's discovery, and D'Herelle's detailed study, of bacteriophage.

Of the second of these instances, Sir Henry Dale said, "Looking back on these observations in the light of present knowledge, it seems curious that they did not arouse more attention. Perhaps the feeling that viruses were intangible and mysterious had produced a reluctance to believe that any bodies which the microscope could make visible, even though obviously small enough to pass any ordinary bacteria proof filter, could represent the virus itself; and it must be admitted that there was no direct evidence of their infective nature, but only of their occurrence in large numbers in cells which had become infected."[1]

It seems very likely that this suggestion is at least in part correct. If it is, this is one instance of how rapidly generalization can follow upon the establishment, or alleged establishment, of properties as related, and of how, when this happens, it may hold up further independent investigation for quite

[1] Op. cit., pp. 10–11.

a long time. But there is another way in which all these three instances—and many more that might be cited from almost any branch of science—are alike. When they were first discovered all of them appeared to be isolated, to be, so to speak, "single" instances of difference. At length bacteria-free infection of mosaic disease was confirmed by more stringent methods and was also shown to occur with "louping-ill," and the original discovery began to be a genuine stimulus to active and new experimentation. When it was proved that fowl pest could be transmitted by innoculation of a single isolated cell inclusion, and when again more stringent methods confirmed the earlier indications about the constitution of the inclusion bodies, the original discoveries became effective in starting off many new researches. When improved technique showed that it is largely the nature of the host cells which gives bacteriophage its lethal qualities the isolated "difference" of the initial observations became potentially linked with many other problems and the active inspiration of much new research.

Tentatively we might say that the mind of the alert experimenter is always on the lookout for points and areas of overlap, between things and processes which natural and unaided observation has tended to treat merely, or chiefly, as different. However, this is one side of the story only. As our illustration suggests—and there are innumerable others which do the same—when overlap of phenomena sets the experimenter to work, and he proceeds far enough to be able to establish causal relationships between events and their properties, he then begins to look round for differences again. Some infectious diseases are bacteria borne; more are bacteria borne; very likely all are bacteria borne. But no; here is one, the mosaic disease of the tobacco plant which is not so. This is awkward, but so long as there is only one disease, and so long as there is, in a broad sense, only one method of study, nothing much is going to be done about it. As soon as other cases are found, and as soon as other methods find them, active work starts up again, and the great chances are that it will have new directions which may set the main stream of research for quite a long time.

Another matter which will strike everybody who studies this particular illustration is that the main periods and lines of

advance seem to come, perhaps most often, from the application to problems of infection of techniques and methods which were basically developed without relation to infectious disease at all: the use of filters; of the microscope, and of more and more exact and delicate microscopic technique; of tissue culture vitally affected by information brought from other fields of chemistry, biochemistry and biophysics; and all the time of statistical devices and methods, particularly as linked with genetical studies. It might seem tempting to say that the greatest part of experimental thinking is concerned with importing the techniques of one field into the practices of another. But this would be misleading, because when any technique is taken into a field where it has not been used before, it has in some ways to be reconstituted to accord with the phenomena of this new field. In this study of infective agents, most obviously of all, only the investigators who knew a lot about the empirical facts already established when they began their work, and were loyal to them, could be effective.

All of this seems to mean that experimental thinking, in this case certainly, and maybe essentially, is fundamentally co-operative, social, and cannot proceed far without the stimulus of outside contacts.

Finally, for the moment, it seems remarkable how often advance in the experimental study of infections has been made possible by steps which have done little but pinpoint the places or regions upon which observation can be most profitably directed. The two most striking examples in this story seem to be the indications that attention should be concentrated upon the inclusion bodies and their elements, and upon the active principle in bacteriophage.

All of these features in experimental thinking can be identified in varied contexts. We will look now at a different kind of instance.

5. THE EXPERIMENTAL STUDY OF REACTION TIMES

I have chosen the experimental study of reaction times as a second topic for the present discussion, partly because it is one with which I can claim at least some direct acquaintance, and

partly because, while it again brings out some of the main points of the case already described, it introduces a few new ones as well. Some of these new points are perhaps of special interest to us because they have arisen in connexion with those recent studies of the nature of skilled behaviour to which reference has already been made several times in the earlier chapters.[1]

For our purposes reaction-time studies can most conveniently be considered in four main phases, or stages, but with much chronological overlap from stage to stage. These were: First, the astronomers' studies of "personal equation" and the development of more and more exact timing devices; second, Helmholtz's experiments on the rate of conduction of the nerve impulse; third, the period of "mental chronometry" proper, and, fourth, in which every human reaction to stimulation is regarded literally as a temporal succession of events, with timing problems arising at each step in the succession.

It is usual to begin an account of the first phase by recording the dismissal of an assistant from his post at the Greenwich Observatory in 1796. Earlier Maskelyne, who was then the Astronomer Royal, had found that his assistant was recording the transit of a star about half a second later than he was himself. The assistant was warned, but in 1796 the discrepancy between the two observers increased, in the same direction, to nearly one second. The subsequent dismissal of the assistant gave rise to a little social and political stir, but the whole affair was, at the time, merely regarded as an isolated difference between two people, and there were no immediate scientific repercussions.

Ten years elapsed. In 1806 there was published, in Germany, a history of the Greenwich Observatory, in which the incident of the dismissal was mentioned. The Königsberg astronomer

[1] There is a very large literature on reaction-time experiments and the related "personal equation" problems. Excellent summaries, with many bibliographical references, are available in Sanford, E. C.: *The Personal Equation*, A. J. Psych., 1888, II, pp. 3–36, 271–98, 403–30; in Boring, E. G.: *A History of Experimental Psychology* (New York: Appleton-Century-Crofts, 1950; and in Woodworth, R. S., *Experimental Psychology*, New York, Henry Holt and Co., 1938). I have also greatly profited from a so-far unpublished paper by Dr. J. A. Leonard of A.P.U., Med. Red. Coun., Cambridge, entitled *A Reconsideration of the Early History of Reaction-Time Studies.*

Bessel was already interested in a general way in "errors" of observation, perhaps through a recently published theory about them by Gauss, and so to him the incident was not isolated, but was at once related to other similar variations between one observer and another.

In 1819 Bessel began, under carefully controlled conditions, to record times of his own reports of stellar transits compared with those of other astronomers. The recording was continued for several years, and by 1830 Bessel had established that there are consistent differences between individuals in the times assigned by them for the observed occurrence of various natural events, had, for certain individuals, measured the range of these differences, and had begun some experimental study of external conditions affecting the assigned times. From the first he expressed the recorded differences in the form

W (Walbeck)[1] — B (Bessel) = 1·041 sec.

and the name "personal equation" was then given by the astronomers to this phenomenon.

For rather more than the next thirty years the studies by astronomers of the personal equation continued. Many details were added and bit by bit the parts played by various astronomical conditions—such as the magnitude of the object to be observed, its direction and rate of movement—were more accurately recorded. But this was, more than anything else, a period in which preparation for the next real step in advance was slowly being achieved by the invention of more accurate recording instruments. The use of a spring pendulum and later of chronographic recording devices made it possible to recognize and measure what was called the "absolute personal equation."

Rather late in this period the interests of other physical scientists, who turned their attention to measures of the speed of travel of projectiles, were added to those of the astronomers in the development of better and more easily used measuring instruments. It was along this line of applied approach that the chronoscope (Wheatstone 1840 and Hipp 1843) came into use, and it was a special form of chronoscope depending upon calculated times from the electro-magnetic action of a spiral

[1] A contemporary German astronomer.

of copper wire upon a magnet suspended by a fibre which largely made possible the next great step forward: Helmholtz's experiment on the rate of conduction of nervous discharge.

It was in 1880 that Helmholtz published the first account of his discovery that speed of conduction in motor nerve could be measured. He knew of the personal equation experiments, though there is no evidence that either then or later he was particularly influenced by any of their detailed results. At first the discovery seemed so radically opposed to accepted views about the practically instantaneous character of nerve conduction, that there was difficulty in publication. But the experiment was one of those which compel assent and objections were speedily cleared away. In fact both the first experiment, with motor nerve, and the later sensory nerve experiments, followed the pattern of personal equation procedure. In the sensory case—perhaps the first reaction-time experiment properly so-called ever to be carried out—Helmholtz stimulated a man on the toe with a weak electric shock, and then on the thigh, and recorded the difference in the reaction time: but he was not very satisfied with this procedure, mainly because he was really interested in the speed of conduction of the impulse through nerve, whereas the experiment involved what came to be known as the "total reaction time." The technique was, of course, later improved with correspondingly more exact measurements.

In principle Helmholtz had, by this experiment, paved the way for the whole development of "mental chronometry" which was to follow. His discovery set beyond any chance of subsequent question the fact that the human reception and response to external stimuli were a measurable temporal series or succession of events. It was to be a long time yet before either the exact nature or the importance of the succession was to be fully appreciated and experimentally investigated, but Helmholtz made both a possibility.

The third phase, sometimes called the phase of "mental chronometry," stretched from the middle 1880's to round about the 1930's; and, in fact, in many ways and places, it continues still. It now seemed that every distinguishable human bodily or mental reaction which could be assigned to the impact

of specific external conditions must take time to get going once the conditions were effectively present, and that the measure of this time might throw light upon the complexity of the reaction, or its level in a hierarchy of reactions, or other important properties of behaviour and experience.

In detail it is neither possible nor necessary here to try to cover this long period, and all I shall do is to pick out the relatively few points which seem to me to be important in relation to the thought procedures of the experimentalist.

Naturally a very large number of different instrumental time measuring devices were proposed and used. But no radically new principles of measuring were found. All the techniques were either direct time recording chronographic or chronoscopic devices, or else were of the kind with which times could be exactly calculated from recorded or measured effects of the exercise of a known force. It is vastly important to realize how much experimental thinking is controlled by experimental method and by experimental instrumentation and how hard it is, once methods and instruments have become accepted and established, to break away from their use. It was almost inevitable that with measurement techniques of this kind any reactions chosen for study should be treated as *single* occurrences, and that if analysis should be attempted this should be a theoretical one, based upon views about behaviour as a whole, combined with recorded differences in the length of reaction times. And this happened. One of the first and most influential of the experimenters to take up reaction times after the astronomers and Helmholtz was Donders, the Dutch physiologist. He particularly distinguished three types of instance: (1) reaction times to "single" stimuli; e.g. a single light flash; (2) reaction times when there are several different "single" stimuli but the reaction must not be made until the particular one concerned has been distinguished; (3) reaction times when there are several different stimuli and a differential reaction for each one of them. The first sort of instance came to be called "simple reaction time" (S.R.T.); the second "discrimination reaction time" (D.R.T.), and the third "choice reaction time" (C.R.T.). As everybody would expect, Donders found that the second was longer than the first and the third

than the second. He now *argued* that "true" D.R.T. was total D.R.T. – S.R.T.; and "true" C.R.T. was total C.R.T. – (D.R.T. + S.R.T.).[1] This interpretation was at first accepted without question and it led to a tremendous lot of frillings of experiment but to no new departure whatever, and to no fresh experimentally accredited advance. Most of the work was done in Wilhelm Wundt's laboratory for experimental psychology which was opened in 1879 and much of it by Wundt himself. Rather later in the study of the times taken by various kinds of "compound reactions" and of largely theoretical inferences from them, there came an experiment by Ludwig Lange[2] which did seem to push into a new field.

Already, fifteen years before this, the Austrian physiologist Exner had reported that the length of a reaction time is in an important way dependent upon the "set," or predisposition, of the observer; but not much had come of this observation. Lange now demonstrated experimentally that when an observer is "set," or predisposed, towards the assigned movements that he must make, if a signal occurs, his reaction time is consistently shorter than when he is "set," or predisposed, towards the nature of the incoming signal itself. He showed also that, apart from any special instructions, some observers naturally adopt the first "set" and others the second. This was the experiment which established the difference between "muscular" and "sensorial" reaction time, and "muscular" and "sensorial" types of observer. Lange had introduced a method of experiment giving a direct measure of internal determining conditions of reaction-time, and if his work had been followed up experimentally it might well have led to a more factual and controlled analysis of reaction behaviour, the basis for which had already been laid down in the Helmholtz discovery.

But Lange's results very quickly got tied up with theory. Oswald Külpe was also at the time working in Wundt's laboratory. Later he was to become a highly influential professor at Würzburg, always with a strong bent towards speculation.

[1] Donders also maintained that there were 12 successive physiological events in the reaction process. But they were all hypothetical and not offered for experimental confirmation.

[2] His paper was published in Philos. Stud., 1888, pp. 479–510.

He used Lange's experiment to discredit Donders' "subtraction" method. Külpe now argued that total reaction processes could not possibly be compounded of elements, each with its own part time. "Set," predisposition, by altering the nature of the observer's task, made, for example, discrimination something *wholly* different from simple reaction, and choice as *wholly* different from both. Külpe's argument, reinforced by interpretations of a lot of later experiment at Würzburg,[1] was very widely accepted, both then and since. It was not really conclusive. All that it justified was the opinion that fractionization of compound reaction times might be a mistake, and that anyway a simple subtraction procedure was most likely misleading. But these conclusions were not demonstrated.

Investigation of reaction time was extremely popular in the experimental psychology of the later nineteenth and early twentieth centuries, and a great variety of interesting and often minute detail was added to the sum of knowledge. But it is true to say that the great bulk of the work was repetitive, with rather minor differences. Nothing really opening up new lines of advance emerged. As Leonard remarks: "On the whole, experimenters were content to take overall measures for a control situation and compare them with overall measures under different conditions."

The next real change, and the fourth phase of reaction-time experiment, began to show itself in the 1930's, was enormously stimulated during the war of 1939–45, and is still in very active progress.

By the end of the nineteenth century even the most unadventurous experimental psychologists were beginning to get a little tired of embarking upon reaction-time experiments which differed only slightly from what had often been done already. This of itself, of course, could lead to no new developments, but it could, and no doubt it did, help to make way for new developments should they come. They did come, and from our point of view the interesting thing once again is that they emerged from lines of investigation which, to begin with, had nothing specifically to do with reaction times.

First, perhaps, in time came the psychological test move-

[1] For accounts of this see Humphrey, G., op. cit.

ment. This, already under way before 1918, was greatly strengthened by many of the practical demands of the First World War. The psychological test is essentially a prognostic device. The idea is that a person's behaviour in some specific situation—the "test" situation—can be used to predict how that person will behave in another, usually a practical, situation. It seemed common sense to hope that differences in reaction times might successfully be used to predict speed, and perhaps accuracy, of behaviour for many purposes of everyday life. As tests, however, all the available reaction-time measures proved to be extremely disappointing. They had some small success especially in the more complex forms, and in relation to behaviour under stress, but by no means enough to warrant their wide use. Some investigators began to wonder whether the measures of reaction time that had become conventional needed to be supplemented by others.

Then, especially towards the later 1930's and after, a general change of orientation towards psychological problems began to become apparent in a good many centres of research. Part of this was due to people who wanted psychology to be of some use in the world beyond the laboratory, and who considered that the kinds of things that had been invented for laboratory observers to do had extraordinarily little relation to most of the normal forms of human behaviour. Part of it came from oustside psychology altogether, from engineers who were interested in communication systems, and others who were developing various forms of self-righting, self-controlling mechanisms with capacities for action very like many of those of a human central nervous system. From all of these directions came influences which stressed the importance of considering sequence in events, as against studies of the properties of responses treated as single events.

From 1930 onwards an increasing number of reaction-time experiments were carried out upon behaviour which was arranged in some kind of series. By itself this was no more than a rather timid break from tradition, but it did at least demonstrate that the time character of response was in fact dependent on the position of the response in relation to a range of near-by responses. More significantly, this was the period

in which the Italian psychologist, Ponzo, discovered the importance of measures of "recovery time"—the interval between the end of one member in a movement series and the beginning of the next.

It was technological advance, which, to begin with, had nothing whatever to do with reaction times, that gave these growing dissatisfactions and their tentative groping out for new experimental problems a positive opportunity. First in the field was the use of methods for studying amplified action currents. This made it possible to treat the physiological events in reaction time as a genuine succession. The really vital technological advance, however, came with the effective development of electronic recorders and computers. It was this which made it possible to carry into practice what was, indeed, theoretically involved in Helmholtz's discovery, that is to treat reaction literally as a succession, to measure the timing of its different phases, and to continue to do this for as long as might be desired. It was established that at least three measures must be distinguished: (1) the interval between the appearance of a signal and the beginning of the response movement (the classical reaction time); (2) the interval between the beginning and the completion of this movement (the movement time); and (3) the interval between the completion of this movement and the beginning of the next movement in whatever succession of movements may be required (the "resting" or "recovery" time). It soon became evident that within a determinable range these intervals are not fixed times at all, but are interdependent, and that in any form of continued action the facts about "timing" are very much more important than are strict measures of time. The reaction-time experiment had at length taken its place in the whole complex of experiments on skilled behaviour, and this is its present position in Experimental Psychology.

6. THE CHARACTERISTICS OF EXPERIMENTAL THINKING

(*a*) *Opportunism*

Anybody who looks back over an experimental development which, as in the case of the two instances I have described,

has continued for many years, can hardly fail to notice that it has pursued an exceedingly wobbly course. If the surveyor is himself an experimenter, he will know that the recorded wanderings are fewer and less extensive than those which actually occurred. Many a time it will seem strange that turning-points of discovery were not reached much earlier. All of this remains odd, however, only to somebody who already has the more complete knowledge which the investigations themselves have gradually built up.

Even when the general problem has a form very like that one of the "transitional" experiments which had to do with "searching in a structured system,"[1] and is concerned to draw a line on one side or the other of which the critical conditions sought must lie, there is no evidence that thinking naturally adopts an economical approach. The identification of minute infective agents and an understanding of their activities was, in fact, of this general type. For a long period the great question was where to draw the excluding line. Experiments proceeded through successive exclusions; but every one of these had only a relatively specialized and limited range.

No doubt whatever "When a subject is faced with a search situation in a structured system he has to realize, if he is to search at all efficiently, that he must use the checks as a means of extracting information. Furthermore, he has to realize that this is best done if he proceeds by asking general questions: 'Is it in this half or that?' in the first instance. To begin a problem by asking 'Is it in this stage?' is a very inefficient procedure."[2]

Given that structure of the system in which search must be made is already known, this is true. But experimental thinking has to deal with systems whose structure begins to appear sufficiently stable—if it ever does—for such questions to be asked, only after much search has been made. The scientific experimenter is, in fact, by bent and practice, an opportunist. He has to build upon the facts—the "sensible" facts as Harvey would say—which are available to him when he begins his work, and he must use them to discover other facts, advancing stage by stage towards that eventual understanding of causal relations which he seeks. To the onlooker who makes a study at an

[1] See pp. 109–11. [2] H. C. A. Dale, op. cit., p. 13.

advanced stage of the work, the experimenter's thinking must often appear wasteful, directed so far as specific stages are concerned, but wandering in relation to final issues, remarkably uneconomical in the sense that what may take the experimenter years to establish may take the expositor only a few minutes to describe.

(*b*) *Method and Instrument*

Both of the instances that we have considered have shown again and again that the experimenter cannot move beyond the point for which methods and instrumentation are available. He may sometimes invent them; more often he adopts them from some source that may be well outside of his own immediate interests.

The experimenter must be able to use specific methods rigorously, but he need not be in the least concerned with methodology as a body of general principles. Outstanding "methodologists" have not themselves usually been successful experimenters.

Similarly, and for much the same reasons, the experimenter must, more and more as knowledge develops, be able to think with instruments. However "pure" his aims may be he has to be able to practise a technique and to handle a technology. Far the most important aspect of the experimenter's need to master method and to handle apparatus is that in the majority of cases—just as in those two that we have considered in some detail—the method and the instrumentation are brought into his field of work from the outside. There is something about experimenting, sometimes even in its routine forms, which demands a variety of interests.

(*c*) *Siting Problems*

At all stages of experimental science one of the principal uses of apparatus has been to aid and improve observation. Also at all stages whenever any experiment is performed there is always a lot more to observe than it is worth while observing. Consequently one of the greatest requirements for successful experiment is to be able to "pinpoint" problems. This is what optics and the use of special lenses and lens systems did

for the very early bacteriologists, what developments of the microscope did for the later ones, and what genetics and radiation analysis are doing in the study of bacteriophage? It is what amplification of very small electrical discharge, and the development of electronic recording and computing instruments have done in the study of reaction-time phenomena. To know where to look, as much as how to look, is a necessary step in experiment. Very often, perhaps always, it is a step that becomes possible when methods, apparatus, hints, or established findings are taken over from some field different from that in which they are to be applied. The identification of problem sites of outstanding concentration and importance has over and over again played a very great part in directing experimental research.

(*d*) *Making Openings*

The experimenter used to be pictured as always on the look-out for a "crucial experiment," and a crucial experiment was regarded as one which settled some question once and for all. It is perfectly true that, like all other thinkers, he is trying to reach conclusions, or terminal positions, which everybody else who has sufficient and sufficiently accurate information *must* accept. But this is neither all, nor is it the most important part of what he is trying to do, except in such cases, much more rare than is often supposed, in which, for the time being, his interests are narrowly practical ones. We saw how the prolonged course of experiments, in each of the cases we have studied, fell into phases, or stages, or periods. At intervals something happened which then set the main stream of research for a long time to come. There is no accident about this; it is the regular course for experimental thinking to take. Original experimental thinking fills up a gap in a manner that commands assent, but also opens up many other gaps either not suspected before or not before susceptible of exact filling which now the more routine experimental thinking can deal with.

One of the most important features of these turning-points in experimental development is that they very often introduce methods and instrumentation new to the field of research involved, but already developed in some other region of investigation. But if the experimenter who does this has any

original impact upon his science he always does more than this. He must adapt the new methods and instruments for use in his own field, and he must show that they can be used to reach a compelling answer to some current problems, and at the same time to lead on to a number of further problems. As we have seen from our two leading instances, and could readily confirm from many others, genuine advance usually means (1) the progressive pushing back of the boundaries of what are regarded as defined fields of investigation, (2) the establishment of smaller and smaller and also, as a rule, more and more units as centres of those causal relations and activities with which the particular scientific discipline concerned must deal, and (3) usually the identification of some of these units and groups of units as possessing differential functions that, in the next phase of development, are most likely to be worth investigation.

For example, Helmholtz brought into the study of nerve conduction the very method already established by the astronomers to measure the difference between one "personal equation" and another. He adapted the method so that he could measure time differences of response within what could be called a single length of motor nerve, and he adapted the apparatus into his own form of myograph. He settled once and for all that discharge along motor nerve takes a measurable time. Implicit in his experiment were a very large number of new questions which sooner or later were now bound to be asked. Those concerning actual times taken by more complex mental processes could be tackled at once, and the "mental chronometry" period which followed was a direct outcome of the astronomer's observations and Helmholtz's experiment. Obviously Helmholtz had left open very important questions of the nature of the nerve impulse and more accurate and analysed measures of its rate. These had to wait for a long time till suitable recording apparatus was available. Equally Helmholtz had left open the whole question of a succession of events within the total reaction time which might perhaps have important bearings upon complex behaviour. These also had to wait, partly because they were obscured by the theoretical wrappings that mental chronometry very soon provided for itself, and partly because, here, too, adequate recording instruments were lacking.

(e) Original and Routine Thinking

The winding progress of any branch of experimental science is made up essentially by a relatively small number of original inquiries, which may be widely separated, followed, as a rule, by a very large number of routine inquiries. The most important feature of original experimental thinking is the discovery of overlap and agreement where formerly only isolation and difference were recognized. This usually means that when any experimental science is ripe for marked advance, a mass of routine thinking belonging to an immediately preceding phase has come near to wearing itself out by exploiting a limited range of technique to establish more and more minute and specialized detail. A stage has been reached in which finding out further details adds little or nothing to what is known already in the way of opening up unexplored relations. In fact the minute improvements in delicacy of measurement, and the like, with which the routine investigator must now be concerned are little, if anything, more than further descriptive items, and science cannot live on description alone.

However, at the same time, perhaps in some other branch of science, and perhaps in some hitherto disconnected part of what is treated as the same branch, there are other techniques generating their own problems, opening up their own gaps. An original mind, never wholly contained in any one conventionally enclosed field of interest, now seizes upon the possibility that there may be some unsuspected overlap, takes the risk whether there is or not, and gives the old subject-matter a new look. Routine starts again.

If these general interpretations of the facts are accepted, I think three things follow.

There is no point in asking whether originality or routine plays the more important part in experimental science. Neither occurs without the other, or can so occur. All that we can say is that for every original mind a large number of routine minds must be set to work.

The conditions for original thinking are when two or more streams of research begin to offer evidence that they may converge and so in some manner be combined. It is the com-

bination which can generate new directions of research, and through these it may be found that basic units and activities may have properties not before suspected which open up a lot of new questions for experimental study.

The thinker in the closed system is in the position of contemplating a finished structure. Very often this may be exceedingly complex and elaborate and the rules of its construction difficult to appreciate. The thinker is, however, in the position of a spectator searching for something which he must treat as being in some way "there" all the time. His search is rational but it is often emotionally sustained, and if it is, the emotion is appropriate to that which is associated with the contemplation of form and beauty of form, and is aesthetic or akin to the aesthetic.

The experimental thinker is in the position of somebody who must use whatever tools may be available for adding to some structure that is not yet finished, and that he himself is certainly not going to complete. Because the materials that he must use have properties of their own, many of which he cannot know until he uses them, and some of which in all likelihood are actually generated in the course of their use, he is in the position of an explorer rather than that of a spectator. His thinking, too, is often emotionally sustained, and if it is, the emotion is one of those appropriate to the chase, to risk, to adventure, and to sport.

These are some of the main features and conditions of the thinking of the experimental scientist in so far as they can be detected by a study of continued experiment, over fairly long periods, carried out by many research workers, on widely defined topics. There are other features, equally important, but better brought out, it seems, by a study of the steps by which a particular experimenter has progressed in developing experiments on some more limited and personal programme. Such a study I shall now try to make.

CHAPTER EIGHT

Adventurous Thinking—3

FIRST HAND ABOUT EXPERIMENTAL THINKING

1. PREPARATION

In a way most of this chapter is autobiographical. It is not only possible, but in the interests of psychology it is necessary to consider mental processes in their general aspects, and apart from any maybe accidental accompaniments of their expression by some particular person. In precisely that manner this book is chiefly concerned with thinking; but there is also the thinker, and we have already had to remind ourselves once or twice that thinking as a process may seem most content with aims and with ways of procedure against which the thinker, as a person, may be inclined to rebel. The characteristics of experimental thinking which have so far been noticed have been selected, in an impersonal way, by study and analysis of records covering long periods of work by many different experimenters. Of the two major topics chosen for illustration, the first is one of which I cannot myself lay claim to any first-hand knowledge, and both go back far beyond any period of my own scientific activity.

It seemed to me important to inquire whether the general characteristics so far assigned to experimental thinking would stand out equally well in a more direct, first-hand analysis, and particularly whether the latter would bring additional considerations into the story.

I am going to try to work out, step by step, the development of my own experiments on Remembering.[1] These were carried

[1] See *Remembering: An Experimental and Social Study*, Cambridge University Press. First published in 1932.

out many years ago, but I have copious notes about them, made at that time, and a great many unpublished records.

It could be argued that these are not very good experiments to take as a model for analysis. They offered no scope to speak of for measurement, even in any but the most simple statistical forms. Their conditions could not be regarded as closely or rigorously controlled. However, some first-hand analysis is needed at this stage, and whether these experiments are good or bad for the purpose, they are the best that I can use.

One thing I must make clear at once. What I am going to attempt is not "introspection" as this would have been understood by the early experimental and general psychologists. I am not, for instance, going to try to say "why" I designed and did these experiments, or to describe through what experiences of emotional or other character I passed as they were developed and interpreted. This is strictly an attempt to trace objectively the steps in an experimental enterprise, although, of course, since these were my own steps, they have to be described in a personal manner.

There is something arbitrary about assigning an absolute beginning to any human event. However, in the Preface to *Remembering* I recalled a sunny afternoon, in the early summer of 1913, when the new Cambridge University Psychological Laboratory was formally opened, and I was assigned the task of demonstrating certain already more or less routine experiments on visual perception. I was fascinated by the variety of interpretations which different people then achieved, all of which they said they "saw," of the same diagrams and pictures; and this was, in a way, the beginning of the whole series of experiments which were to occupy a main share of my attention for years to come.

As I now look back, it is clear to me that I gave then less consideration than they deserved to some preceding years of informal and, so to say, "undesigned" preparation. Most of my earlier academic preoccupations had been with exceedingly general philosophical and logical problems, and with what was then accepted as formal logical technique. By now I was already beginning to move away from these, though this may well have been—it is of little moment for my present purpose—because

I was rapidly coming to the view that it was beyond my power to do much to answer such problems in any original way. My immediate close friends, with whom I had prolonged walks and talks, were mostly scientists, with a mathematician or two, an historian, and a philologist and student of French literature. I had also come closely under the influence of Dr. W. H. R. Rivers, the anthropologist, and of Dr. C. S. Myers, who was at that time the Head of the Department of Experimental Psychology at Cambridge.

As I look back now, it seems to me that although it may have been the oddity of the results of my demonstrations on that opening day of the Laboratory that led to the first steps of the experiments on Remembering, it was the mixture of preparatory influences and interests that gave them their early direction, and whatever originality they may have achieved.

2. FIRST STEPS

Every experimenter must start where somebody else has left off. I agreed that earlier experiments had demonstrated the active character of perceiving, and I used material, most of which was to hand, and had been employed by other Cambridge experimenters already. This material, consisting of diagrams, line drawings, and black and white and coloured pictures, was exposed with controlled illumination, length of exposure, and so on. The observers gave an immediate report, or when they could do so, themselves drew what they alleged that they had seen. This was all on a conventional level and the only point worth paying attention to at this stage is that I was looking for ways of demonstrating experimentally those parts particularly of the complex perceiving response for which the observer himself was directly responsible. What set out, then, to be an examination of perceiving very quickly moved towards becoming a study of remembering.

My first decisive step away from complete conventionality of experiment came from outside. I was expected from time to time to report my results to Professor James Ward, and once when I did this, with some emphasis upon the memories which my observers had brought with them into the experimental

situation, he said, "Yes; well, you might perhaps have done something worse. Why don't you use designs and things in series, with one linked to another and leading on to it in some way? It doesn't seem that experimenters have tried this very much, but it is what happens to us in perception every day."

My notes suggest that what was in his mind was that it would be possible to make developing series for exposure which varied consistently in one or more selected dimensions, and by doing this to reach a reasonably controlled understanding of how, and under what circumstances, the dimensions chosen influenced the activity of visual perception. In particular it appeared that he hoped that it might be possible to establish accredited general principles about the functions in perception of agreement (or overlap), and difference.

I know now that it was a long time before I fully realized the importance, for many psychological experiments, of putting the situations which are used to produce response into a sequential form. That I began at once to use this form for the perceiving experiments was due to Professor Ward.

Obviously the number of dimensions of change that can be introduced in a controlled manner into diagrams, decorative and pictorial representations, and worked into sequences, is very great indeed. Anybody who cares to look at the final report of the perceiving experiments will find that only a very small number of these were mentioned, among them such features as position of change; number of "items" changing; critical regions, in a sequence of changes towards an end-point accepted as meaningful, or appropriate, or approved. Only when I dig into the large stores that I still have of reports of sequences of change in special dimensions of display, not for vision only but for most of the other senses also, do I realize in what a wandering way I followed this course of exploration.

We saw that an experimental search, carried out by a lot of people for a long period, need not set enormous value upon economy of effort and great directness of aim. And certainly in this particular research, and indeed in nearly every other that I have tried to make, the number and the range of results which I ultimately used were small indeed compared

with those which I obtained and recorded. I shall return to this point later.[1]

3. A CHANGING PROGRAMME

The use of material in sequence for my experiments was, of course, almost bound to push the scheme of work still farther in the direction of a study of remembering; for every successive member of the sequence was seen in conjunction with whatever could be recalled—in the functional sense—of preceding members. It was now that I began to plan seriously for two different approaches, treating one as something to avoid, and the other as something to try to do.

It is a common thing for experimental thinking to be powerfully influenced in the direction of doing something different from what acknowledged authorities have done before, though naturally it is not only the experimenter who is prone, on occasion, to take this line. Most people who have told the story of Helmholtz's discovery of a measurable rate of nerve conduction, have pointed out that Johannes Müller, Helmholtz's teacher, had declared not long before that any such measure was impossible, and have suggested that very probably this was one of the influences that set Helmholtz searching. Like everybody else, then and since, who has had a training in experimental psychology, I had received abundant information about the brilliant work of Herman Ebbinghaus and his successors. For their remembering experiments they generally used nonsense syllables in lists. Apart from certain formal rules which had to be observed, there was no reason why any syllable in any list should not change place with any other syllable. These were not the sorts of sequence which my perceiving experiments had been using, and for that and various other reasons I decided to avoid any such approach.

This negative influence came from earlier work in the very field which I proposed to explore, but the more positive influence came from outside that field, from anthropological interests, though, at the same time, it is true that the results of some of the experiments I had already done fitted well into these. In

1 See p. 183.

several of the perception sequences it seemed that there came a stage when something like a stored pattern or standard representation took charge of the observer's response and principally settled what he was to allege that he had perceived. Moreover, observers from much the same social group were very likely to use the same stored standard representations.

Just about this time I had become interested in ethnographical studies of the development of decorative art forms, such, for example, as those undertaken by Dr. A. C. Haddon in *The Decorative Art of British New Guinea.*[1] It was from such writings that I borrowed the plan of attempting to devise and use sequences for perceiving and remembering so as to help towards an understanding of those processes of conventionalization which not only produce standards and patterns peculiar to the decorative art of a social group, but could also, it seemed to me, play an important part in the individual's interpretation of his own environment.

The programme was now moving away from straightforward studies of the determining activities and conditions of perceiving and remembering, towards an all out experimental attack upon conventionalizing, in both its individual and its social forms. When now I look back over many volumes of old notebooks, I am surprised to find how widely I wandered into comparative studies in a search for material that might be suitable for experiment, and for those dimensions of transformation which might be most worth studying for their part in conventionalizing processes. Once again, and not for the last time in this particular story, the course which I followed was anything but a straight one.

In its ultimate issue, my research was to conform fairly closely to its earlier programme, and though processes of conventionalization were to receive a good deal of discussion, this was to be less closely tied up with the experiments than at the stage I am now describing I had hoped would be the case. I possess a copy of a long essay containing much of the material which found a place in *Remembering*. It is entitled *A Contribution Towards an Experimental Study of the Process of Conventionalization*. The publishing agreement which was made between the

[1] *Cunningham Memoirs No. 10*, Royal Irish Academy, 1894.

Cambridge University Press and myself was for a book on "Conventionalization." There came a time when I began to write this book, and I laboured heavily through two or three chapters, but it did not go well. I tore up what I had written and for some time there followed a most unpleasant period when it seemed that I had taken a lot of steps to get nowhere at all.

Long before this, and while I was still in the "conventionalization" phase, another suggestion came to me, also from outside, about a new possible variation of method. Norbert Wiener, who has since become famous for his original contributions to mathematics, and for his development of what he calls "cybernetics," was at that time in Cambridge to study mathematical logic. We became close friends, and had tremendous arguments with one another. One day, when I had been talking about my experiments, and the use I was making of sequences in a study of conventionalization regarded as a process more or less continuous in time, he said: "Couldn't you do something with 'Russian Scandal,' as we used to call it?" That was what led to the method which I later called "The Method of Serial Reproduction," one which, in varied form, was to contribute much to the final working out of my experiments. Once again a turning-point in these experiments was reached by outside instigation, and was not directly achieved by my own practice or my own reflexion. And once again it was a method of approach that was important.

4. AWAY FROM THE MAIN TRACK ONCE MORE

Some of the perceptual sequences which I had designed were seen as progressively nearing a final representation or pattern, perhaps by the addition, perhaps by the omission, of detail in successive displays. Many observers then "constructed" a terminating design before they had reached it, and having done this reported seeing detail which in fact was not, at the time, present at all. Even more, the same constructive kind of treatment was given to all the sorts of materials which I used when I passed on to the different types of experiment on remembering. The observers were constantly "filling up gaps" without waiting for the confirmation of external stimulation.

In the general psychology of the period "construction" in mental process was always regarded as a function of imagination and of thinking. Moreover, Külpe's group at Würzburg had pretty well achieved the termination of their attempts to develop an experimental approach to a study of thinking a few years earlier. It seemed that here was an important movement to which I ought to pay attention, and "paying attention" in this case meant not only reading vastly long and elaborate reports, but also devising and doing a lot of experiments closely following the German models. My own conclusions after months and months of work, so far as they were relevant at the moment, were (1) that, given their methods, nobody would be likely to get any farther or do any better than the Germans had done; (2) that the methods were not, by themselves, adequate for anything approaching a controlled study of thinking processes.

What I learned then was extremely useful to me later on, but it contributed nothing to the work with which I was at that time principally concerned. I record it here as another instance against the view that thinking, when it is effective, is bound to be narrowly directed, to favour short cuts, and to adopt everywhere the style and method most suited to certain "closed situations."

5. FINAL STEPS

Nearly every experimenter who has tried to do something more than to repeat experiments that have been done before, perhaps with some minor alteration in method or aim, must have found himself confronted by a mass of detailed results, and wondering how in the world he can reduce them to some semblance of significant order. He may, as I did, start trying to do this several times over, and every time find that he is on some wrong tack, and have to give up. He may even dismiss the whole effort, put it away, and then, coming back to it later, hit upon some clue which gives order and direction to the mass of material that seemed without structure.

In this particular instance there were some special difficulties. Hardly were my experiments properly under way, when the war of 1914–18 broke out, and I was quickly caught up in a

number of other activities, and many of these, added to in various other ways that had nothing to do with the experiments, continued for long after the war had finished. The research and my thinking about the research had to pursue an interrupted course. Apart altogether from these special circumstances, however, there is the fact that many an investigator has recorded how, having to hand all the materials which he is going to use to fill up gaps in knowledge he has seemed to himself to find the clue to the use of the material in such a way that it will convince, or attract approval, only after he has stopped thinking about the details for a while, and very likely turned his attention elsewhere.

During the later stages of the war, and the years immediately following, Sir Henry Head was carrying out his experimental work with aphasic patients and developing it for publication. We met frequently, and it became his habit to read over to me the chapters of his projected book as they were written. I cannot pretend that I contributed much to them, but it is quite certain that from his reading of these chapters, and from much resulting discussion of his earlier work on afferent sensitivity, I got the clue which I wanted for putting into order my own different collection of detailed experimental results. Head was profoundly interested in the ways in which the peripheral and central nervous systems dealt with their "stored" results of past responses. He considered that he had irrefutable experimental evidence that past responses continued to have a direct functional significance in the shaping of present behaviour, not as stored individual traces of reaction, but as organized response groups to which he gave the name of "schemata." That I might take over and use "schemata" as a chief clue to reduce to order what often seemed to me to be the tangled mass of my own results, was not, at any time, a suggestion made by Head to me, and unfortunately, owing to his rapidly failing health, I was never able to discuss the matter properly with him. It was an importation from one field of experiment into another one.

Several critics have pointed out that the importation could not be effective without some change in the treatment of the master clue. No such importations ever can be. Whether the changes I made were justified or even fully intelligible, are not

at the moment in question. When we were reviewing in the more impersonal way the two long stories of experimental investigation which passed on from thinker to thinker over generations, we found that critical moves in progress seemed most likely of all to come when different fields of experimental adventure were brought into contact. In the same kind of way, what made it possible to take the last step to a temporary termination of the long research into Remembering, came through contact with a field of study different from my own.

It may well be that it is this need to bring instrumentation, methods, and ideas from one field of study to another that often seems to make it difficult for the experimental thinker to get on with his work effectively unless now and then he turns away from his own special field, and even seems to stop thinking about it altogether. Perhaps all original ideas and developments come from the contact of subject-matter with different subject-matter; of people with different people.

6. ANSWERS WHICH RAISE QUESTIONS

I can say quite categorically that when, in 1913, I began my experiments on Perception I did not, and so far as I can now judge I could not, foresee the long course which they would take, or how each halting-place, as I was to reach it, would open up new lines for exploration. I suppose there is a sense in which reflexion upon the general character of perceptual processes would be fairly likely to link these processes with imagery, and imagery with recall. But it was not as a result of any general analytical theorizing that the experiments followed the course they did. I have already emphasized the fact that the actual course which I took twisted and turned a lot, and that the bulk of those twists and turns found no place in the final exposition. It is, however, true to say that each recorded step of experiment was performed in the order in which it was expounded, and that when I passed from perceiving to imaging, from imaging to remembering, and from one kind of remembering experiment to another it was with questions in mind that the preceding stage of experiments had specifically raised. The whole process appeared as one in which gaps, progressively

filled, opened up fresh gaps, and no really final halting-place was reached. So, when I had at length come to what I was prepared to regard as an end of the remembering experiments, it seemed to me that they had brought me face to face with a whole set of questions about constructive imagination and thinking, which still called for answers. All the way through my course seemed every now and then to reach resting-places, but it never got to a real full stop.

7. THE SELECTION OF EXPERIMENTAL VARIABLES

Probably any experimenter will be able to provide cases in which it became very important for him to decide which of a number of possible dimensions of change he ought to concentrate upon for the purposes of his experiments and what order of priority he ought to give them. The experiments which I am considering here are not very well adapted to bring out clearly the difficulties that may arise at this point. Material which can be used in experiments on perceiving and remembering can usually vary in a very large number of ways, so large indeed that no experimenter could possibly work through them all, and the practical questions become those of deciding what sorts of variables can be left out altogether, and how those that are to be studied can so be weighted as most clearly to show how they produce their effects.

I have already made it clear that in all these experiments I played about with many more variables than found any place in the final exposition. It is not difficult to account for the selection of those that were eventually given detailed treatment: they were the ones that yielded the most unequivocal, decisive, and uniform experimental results, and also opened up challenges to further experiment. But it is very much more difficult, with material as complex as that which I had to attempt to use, to be at all sure about what sorts of thinking usually lead a successful experimenter to pick out the dimensions of change which he will control, before in fact he has any results about them at all.

Some twenty years ago an experimental psychologist who had a great and deserved reputation for the originality of his work became interested in the possibility of using other sensory

modes besides hearing for language communication. He pointed out that while all manner of experimental studies were available about most of the modes of sensation, vibration sense had been given small attention. He set out to discover whether vibration stimuli could be used, especially, perhaps, by seriously deaf people, for intercommunication. They can obviously be made to change, in any way desired, in five main dimensions: in frequency; in intensity, or amplitude; in duration; in wave complexity; and in locus in space. Whatever units of communication are then chosen they can be coded in an appropriate manner in terms of whatever dimension or combination of dimensions has been picked out for study.

The pioneer experimenter chose to rest his case on a controlled study of the frequency dimension. For this there were excellent reasons: the analogy of auditory communication behaviour, and the fact that the instruments available at the time made exact variation in frequency of vibration the easiest to control. Initial experiments were not very promising. Continued experiments seemed to progress but little. The experimenter still insisted that his comparative lack of success must be due to something wrong either in the instrumentation or in the technique of the experimental approach. He and his immediate successors continued to work with frequency changes. Their results improved, but not very markedly, and after a while the project was discontinued. About 1955, when it became practically urgent to consider possible means of communication under conditions in which ears and eyes are already fully occupied, the matter was raised again. Professor Geldard and his collaborators were then able to show that vibration stimuli can be used with great promise of success for certain communication purposes, but that, of all the possible dimensions of controlled change, frequency is the least sensitive and effective.[1]

This is one of the many matters for which further and more detailed studies would be welcome. Certainly it is not difficult to find many other cases of experiments which have failed because an unsuitable variable has been pitched upon for study,

[1] See Professor Frank A. Geldard's brilliant lecture entitled *Adventures in Tactile Literacy* delivered at Chicago, Illinois, in 1956, and published in American Psychologist, 1957, pp. 115–24.

and the experimenter has blamed a method, or an instrument, and gone on looking in the same direction, when it would have been better if he had confessed an error and made a new start.

When an experimenter is thinking out what variables he will principally rely upon in the development of his work, he must be strongly influenced by two sorts of consideration: what instrumentation is available to him at the time, and what use he can make of the nearest analogous case to his own. Both demand accurate and very often extensive factual knowledge. But they are not enough. They will give the experimenter his positive start, they will not by themselves help him if he gets into trouble. Above all else, the experimenter must be adaptable, and that means that he must not be afraid of admitting mistakes, and that he must be as ready to drop an experiment as to take one up.

These seem to be in form precisely the same qualities that Jacques Hadamard considers are required of the original thinker in mathematics. He says: "Good mathematicians, when they make errors, which is not infrequent, soon perceive and correct them. As for me (and mine is the case of many mathematicians), I make many more of them than my students do; only I always correct them so that no trace of them remains in the final result. The reason for this is that whenever an error has been made, insight . . . warns me that my calculations do not look as they ought to."[1]

Perhaps the mathematician, or thinker in any form of closed system, who quickly stops doing something that he has begun in error, has something like a pre-perception of the "fitness" of the structure which he is building to that of the complete structure within which he is working. Exactly how or through what mechanism such pre-perception can be achieved is still exceedingly hard to understand; but the process is, in function, precisely that of the "matching" which I described and illustrated at simpler levels in Remembering.[2]

Whether the experimental thinker in a natural science is,

[1] *The Psychology of Invention in the Mathematical Field* (Princeton: University Press, 1945, p. 49).

[2] See especially pp. 20, 44–5, 195–6.

ultimately, in any different position can be debated, but to me, at any rate, it seems clear that his immediate position is not the same. When he gives up a certain line of experiment and tries another one, it is not because he pre-perceives that the results of his experiments fail to "fit into" a system treated as complete already. It is rather because he suspects strongly that if he goes on he will come to a dead end, or to some terminus where he can do nothing but circle around without further progress. While the one may depend upon some superior sensitivity to formal features such as shape, and volume, and balance, the other relies rather upon superior sensitivity to the proximity of insuperable obstacles, or to a welter of openings so numerous that if he reaches them he is unlikely to be able to do more than wander aimlessly. In neither case, however, can it possibly be this kind of superior sensitivity alone which is needed. Both the formal and the experimental thinker must also remain attentive to the particular results achieved by each step as they take it, for without this they might know that they were in error, but not what to do about it.

Sir Henry Head used to say that there are two sorts of young scientists. One can be led into what seems like a forest, and left alone. He will find for himself a path and follow it, though not without winding; and after a while he will get himself out on to a broad highway. The other can be set upon a well-marked track, and left alone, and he will pursue some by-path and go on and on and eventually he will end "up a tree."

Can it, at least partly, be because the first is willing and able to make predictions and the second is not?

8. PREDICTION IN EXPERIMENTAL THINKING

It has frequently been suggested that prediction is the most important of all functions in experimental thinking. The experimenter, having reached a position from which he can launch an experiment, makes some more or less general suppositions about the constitution of affairs at that position and then designs an experiment, saying: "If my suppositions about the constitution of affairs are correct the results of this experiment will be so and so." Should the results turn out as expected,

the experimenter, it has been said, may treat his suppositions as established.

Experimenters who believe that they have already achieved a knowledge of all the general principles that they need for explanation, so that they can consider themselves as working strictly within a "hypothetico-deductive" system undoubtedly do proceed in this way. So do many others who have a far less articulated appreciation of what they are doing; but this is not the kind of prediction we were looking for when we asked how an experimenter decides whether particular lines of exploration are worth following or not. If it is accepted that this is the way in which the experimenter must use prediction, then prediction may be a device of method useful in the effort to close some gap, but it cannot help the experimenter to tell whether the closure effected will permit any further moves, or if it does whether the moves will lead to a hopeless multiplicity of possibilities.[1]

This is perhaps the place at which I should mention a confusing occurrence which most experimental scientists must at some time have noticed in connexion with their own work. I will quote from a letter written to me by Dr. E. F. Gale: "I have," he says, "been struck by the frequency with which results are obtained which support hypotheses and then, at a later date, cannot be confirmed either by the worker himself or others. My own field of investigation is one of the few that are left in which the way is completely open: no one knows how proteins are synthesized. There are many theories and the history of the subject during the last twenty years contains many examples of experiments which confirm particular theories —and then can never be repeated. We never know whether these results are due to particular but unknown conditions being present on specific occasions, or whether, as you suggest, the experimental design is at fault."

[1] There are, of course, other difficulties of a more logical and methodological order. That results of experiment conform to suppositions made before experiment may show that they are consistent with such suppositions, but cannot by itself establish the suppositions as the only correct ones. Moreover, once an experimenter gets into the way of treating his subject-matter in this hypothetico-deductive way, it is all too easy to design experiments so as heavily to weight them in favour of the "wanted" results.

As might be expected, I have often noticed this sort of thing in my own experimental work and that of my students. According to my observation it is most liable to occur in early stages of the experimenter's approach to what is for him a relatively new field, and although it is not confined to these, I have come regularly to distrust the results of early experiments. I am certain also that when this has happened in cases which have come under my direct observation, the experimenter—myself or another—has been "trying out" some hypothesis, and the hypothesis has been given a higher order of probability than strict regard for the available evidence has warranted. I suspect that this principally means that other possibilities have been neglected, either in detail or as an "unarticulated batch." We have seen from our experiments on thinking that what the thinker treats as empirically probable is often far short of what he would have to consider as theoretically possible.

If the number of the hypotheses which an experimenter will regard is fewer than the number for which there is at least some suggestive evidence, he must assign a higher degree of probability to the ones he does take into account than is perhaps justified, and this will affect not only the design of his experiment but also the inevitable selection that his observation effects from among the results of his experimental trials. It may be a very good rule for an experimenter not to wait when he gets what he takes to be a good operative hypothesis, but to design experiments before he loses the flush of his first enthusiasm. But it is a still better rule for him to put aside the early experimental results for a while, and later, when his enthusiasm has cooled, to try the same tests over again.

If hypothesis must direct experiment there is also something to be said for entertaining more than one hypothesis for each experimental design. But this is not always easy to compass.

There is a different set of conditions which we must not entirely neglect. When we go on to discuss "everyday thinking" we shall find that once a gap presents itself to be filled, the thinker is, in this realm, apt to accept some social convention, or to generalize from some personal experience, and to pass, without further ado, to sweeping assertion. All kinds of

everyday style. A hypothesis is in some respects like a convention. Perhaps, through the lure of priority, or something of that sort, the experimenter may at times overstate his case, but not knowingly or with intent.

We should look, however, for other ways in which prediction is used in experimental thinking. Sir Henry Dale, in the lecture already referred to, gives two instances, though he calls them "prophecies" and not "predictions." Speaking of Huxley's 1870 Address he says: "Huxley went far indeed in the direction of a prophetic forecast of that rapid advance in the knowledge of bacteria, as self-reproducing causes of infection, which was one of the greatest events, for biology as well as for medicine, of the following three decades." Later, when he is himself considering the state of knowledge in 1935 concerning the issues of Biogenesis and Heterogenesis, he says: "Our choice . . . must have the character of a prophecy rather than a conviction, dealing with probabilities rather than with established facts; and in making such a prophetic choice I think we may properly follow Huxley in giving due weight to historical analogy. Biogenesis, as Huxley has told us, has been repeatedly on its trial, under increasingly difficult conditions of defence; as attention has been directed to smaller and smaller living units. Hitherto it has won at each successive stage."

It might seem as if these two general forecasts are advancing nothing at all except that studies of the infective properties of extremely small agencies will be worth making, and as if precisely the same upshot could have followed if the opposite side in the controversy had been supported. But this would be incorrect. Huxley picked out Jenner's vaccination discoveries and malignant tumours as two fields particularly worth investigation, and Dale himself pointed to bacteriophage studies as promising lines for exploration.

An instance in point from the reaction-time story was Külpe's interpretation of the Lange experiment. This contained no prediction in the sense that it forecast what would happen if any particular experiment were tried. What Külpe did assert was that among the many things that might be done, starting from reaction-times, one of the most profitable would be to thinkers are from time to time in danger of falling into the

follow up all lines of experimental study of the influence of "preparation" and predisposition, without assuming beforehand that the results must be submitted to a strict additive and subtractive treatment. It was this which opened up all the Würzburg developments, and although these did appear eventually to reach very much of a "dead end," before they got there they produced a great amount of interesting work and raised issues that are still challenging research.

When I try to recall instances in which I have myself used a form of prediction in the interests of experiment, I find that most of them have taken the same rather general character of pointing to some line of experiment as one specially worth following. When I was doing preliminary experiments for the work on perceiving and remembering, I noticed that certain specialized difficulties were apt to be found in preserving an order of original exposure. It seemed to me then that this might be linked up somehow with earlier work on the direction of associations, for associations, as all psychologists now know, are found to be readily reversible in some cases, and not reversible at all in others. I suggested that remembering order in a sequence, either in action or in words, ought to be a line of experimentation that it would be fruitful to open up, and that in particular attention to the media of original presentation might well be important both in individual and social studies of remembering. Another instance, belonging to an early phase of the Cambridge work on bodily skilled performance,[1] was a forecast that one of the most effective ways of reaching a better understanding of the nature and conditions of animal skill would be found in experiments on "timing" and on the temporal structure of behaviour. A third case is the prediction that, of the characteristics amenable to psychological experiment in the study of ageing, "anticipation range" and "point of no return" will particularly repay study.[2] It seems fair to say that the first two of these forward-looking claims have already been substantiated, though there is much yet to be done; the third still remains little more than a prophecy.

[1] See Ch. 1 and the references there cited.

[2] See, e.g., *Ciba Foundation Colloquia on Ageing*, Vol. I (London: J. and A. Churchill, 1955, pp. 211–13).

In the last two cases, I could draw upon prolonged and fairly wide experience, and upon practice in the experimental fields involved and related ones. In the first instance, my experience and practice were much less, but the suggestion was based upon experiments of my own, and on what I accepted as a likely analogical argument from earlier experimental work. Perhaps experimental thinking gives more weight than most kinds to analogy and overlap. At least when it risks the kind of forecast which opens up avenues of study it never does so on general principles alone.

Yet it is equally certain that something more is needed for good forecasting than wide knowledge and the successful practice of experiment. These must be combined with willingness to take a risk, and to move from evidence which opens up a lot of possibilities in some order of preferred direction. Because varied possibilities are always a part of the story, the forecasts must allow for adaptability in practice, and any experimenter who pursues them in the spirit in which they are framed must know early when to move away from one line and into another.

Dale spoke both of Huxley's prophecy, and of his own, as based in some way upon probabilities. It may be so, but if it is, they are "probabilities" which usually are not, and in fact at the time at which they are being used absolutely could not, be stated. They are not like those in the "closed systems" which we have considered, whose number and weighting can be determined, either rationally or empirically. Their number and weighting will vary as the experiments which lie along the line of the prophecy are carried out, and especially as the instrumentation for such experiments is developed. It is more psychologically correct to say that prediction in this sense depends less upon assessing probabilities than upon positively picking a line of advance as most "likely" to be fruitful. When everything has been said that can be in terms of extent and accuracy of knowledge, and practice in experiment, it still looks as if the successful use of this sort of prediction depends upon great sensitivity to positive properties of direction in the contemporary scientific movements, most frequently as these exhibit overlap with earlier movements out of which they have

grown. When the forecast is made it is usually difficult, and often impossible, for the forecaster to say anything at all about the cues which he is using. If, however, he is able to compare practical lines of possible experimental development, and to assess in terms of probability either the odds that any of them will be followed, or that any will be successful, he must be able to identify at least some of his cues, and he must allege some knowledge about their relative weights. We come to this: that the capacity to identify ahead of anybody else, lines of experimental development which are "likely" to be fruitful, or "likely" to fail, depends upon evidence; but that it is by no means necessary that the person who uses this evidence should be able to say what the evidence is.[1]

Broadly, then, there are two ways in which experimental thinking may resort to prediction. One, depending upon some special sensitivity to trends and movements in a whole experimental field, picks out lines of development "likely" to be fruitful, or to meet insuperable obstacles. The other kind is based upon argument, deduction, and uses experiment as illustration. This way of predicting may take two forms. One assumes that all the general principles required for the understanding of particular operations, or relations, are known, and may design some novel experiment "predicting" that its results will provide an example of how the principles work.[2] Or again, it may accept a plurality of working principles and use either incomplete knowledge of structure or empirical distributions

[1] The only thing peculiar to scientific evidence about this is the field in which it is exercised. The same thing happens with many other kinds of forecast. In *The Last Grain Race*, Eric Newby (London: Secker and Warburg, 1956), the author, records (p. 179): "Thirteen days out from Spencer's Gulf we crossed the 180th meridian and suffered two Fridays in succession. . . .

" 'Koms to blow,' said Tria to me on the afternoon of the first Friday as I came on deck to pour the washing-up water over the side.

" 'Good.'

" 'No, no. Not good. Koms to blow bad,' he replied anxiously. I asked him how he knew.

" 'I don't know how I know. There's someting fonny, someting noh good in the vind.' "

[2] For an interesting current instance of this kind of prediction see "The benefits of additional kinaesthetic and visual information in locating objects first shown peripherally." C. B. Gibbs and A. H. Tickner (Applied Psychology Research Unit, Cambridge, Report No. APU.291).

derived from a large number of experiments and observations, to predict limits of probability within which, in some new instance, one or another result may be expected to occur. Both of these two forms are possible only when experiment has reached a fairly developed stage, so that at least some parts of its subject-matter can be regarded as belonging to a "closed system."

So far as I can see, the confirming value of prediction for experimental thinking in the last two senses has been heavily overrated, and far more scientifically interesting results usually follow their failure than their success. But as to this there is likely to be difference of opinion.

9. SOME QUESTIONS OF MEASUREMENT

Everybody who discusses experimental thinking, whether from a logical or from a psychological point of view, sooner or later is bound to come upon questions concerning measurement. Perhaps this is because the world of empirical relations with which experiment must try to deal is in fact literally a world of quantities is, in a basic sense, a closed system, although the experimenter is rarely, or never, able to draw sharp boundaries to show either absolute or departmental limits in any precise manner. Perhaps it is because, as we have suspected already, thinking itself is always hankering after a world in which the relations of the constituent units can be expressed in terms of numerical and geometrical analysis and construction. Perhaps it is because experiment cannot go on very far without making use of a great amount of stored information, and the most effective way we have of storing information is to put it into some kind of numerical code. Whether the experimenter moves towards measurement because that is what the nature of the world compels him to do; or because it is what the inescapable preferences of his thinking activities compel him to do; or because it is the price of maintaining scientific continuity—these are questions which cannot be resolved on psychological considerations alone. What nobody can possibly controvert is that in every branch of experimental science up to now, as advance

has been achieved, the move towards measurement has taken place, and has become more and more decisive.

Even my experiments on perceiving and remembering around which a good part of this chapter has been written, and as to which I thought it necessary to point out that they offered little scope for measurement, could be used in illustration. The experiments showed the importance of "dominant" detail in perception and in recall, and made it possible to specify certain classes of detail that could be called "dominant." Among them were "gaps" in a visual display. They were very likely to be noticed and recalled, and the more or the less likely according to their position in the field of display within limits of size. A gap high up in the left-hand quarter of a field of display was, for example, most likely of all to be "dominant." The experimental observation has been confirmed again and again. It is a case of a relation which is "so obviously of statistical significance that there is no need to calculate the critical ratios."[1] However, it might become very important to calculate these. According to the experimental results when different classes of dominant detail are present together in a visual display, they influence one another, and both their liability to reappear in perception and recall, and the form of their reappearance are changed. To make any statements from which deductions may be drawn now demands an assessment of probabilities and some determination of their limits of significance. As things are at present, those probabilities can be arrived at empirically only, and so other statistical questions arise concerning the number of experimental observations required, the time limits within which experiments treated as belonging to the same group must be made, and so on. It is theoretically possible that if we had exact knowledge of the receptor systems that are activated in these cases, and of their capacity, and if we knew the number of stimulus items for the response sequence to any class of dominant detail, we might be able to state probabilities abstractly and without resort to a large number of experiments.

[1] Professor J. H. Gaddum: "Any result that is not of statistical significance is likely to be untrue, but sometimes it is so obviously of statistical significance that there is no need to calculate the critical ratios." Ciba Foundation Symposium on *Extra Sensory Perception* (London: J. and A. Churchill, 1956, p. 229).

But it is quite certain that we do not yet know these for any kind of human behaviour response above the level of simple sensory threshold determination, if even there. Most of biological science and not a little of physical science is in the same position. Experimental thinking, then, requires not only adequate technical background to select (and in most cases to use) appropriate instrumentation. It may also need to have enough specialized knowledge to identify (and perhaps to carry out) appropriate statistical procedures. Once quantification of properties and relations is effected in experimental thinking, whether in a statistical or in any other sense, any further thinking about such quantities is at least in part deductive in nature, belonging properly either to the "closed system" or to the "transitional" kinds that we have already considered.

10. THE PRINCIPAL CHARACTERISTICS OF EXPERIMENTAL THINKING

I can now attempt a summary statement of the main characters of experimental thinking as they have appeared in the last two chapters.

1. Experimental thinking comes as a relatively late development in the search for knowledge of the world, since it has to be based upon much prior accumulation, description and classification of observed facts, and upon the invention of special methods and usually special instruments for establishing controlled sequences among these facts.

2. The basic challenge to experiment comes when events and phenomena which have appeared to display differences to immediate observation are seen, or suspected, to possess overlap and agreements. Experiment sets out to account for the overlap, to determine its range, and to find where once again it breaks out into differences.

3. Experimental thinking from the beginning and all through submits to empirical control. It is therefore much more an expression of specialized interests than is any other sort of thinking that we have considered so far.

4. Because of its empirical foundation and control, it is opportunist by nature. It must attempt to deal with highly

complex situations and systems, the structural properties of which can be found out only as experiments proceed. In consequence both the individual experimenter, and the broad course of experimental progress itself, are apt to take a wandering course, and when temporary, or final, issues are reached the course taken generally looks to have been a very uneconomical one.

5. Much experimental thinking has to be in terms of methods and instruments, and this becomes more marked as, in any field of exploration, experiment itself develops. Over and over again the most outstanding scientific advances have been made when methods and instruments invented to deal with one set of problems have been taken over into areas with which they had little or nothing to do in their origin. This again means that successful experimental thinking almost always demands multiple interests, and it has much to do with the fact that experimental discoveries are often made long before they are actively developed.

6. Especially in early stages of specific experimental progress, some of the most important thinking of the experimental scientists concerns the pin-pointing, or siting, of problems; this may do more than anything else to save the experimenter from losing himself in detail.

7. Like other forms of thinking, the experimental kind is set to reach issues in a step sequence which will compel assent from all normal people who are prepared to accept the experimental approach. But it is not satisfied with this; it must also achieve new openings.

8. The most important of all conditions of originality in experimental thinking is a capacity to detect overlap and agreement between groups of facts and fields of study which have not before been effectively combined, and to bring these groups into experimental contact. Any experimental science is likely to show successive prolonged phases of advance and consolidation as it progresses.

If we consider the case of the individual experimenter, the following appear as important determining influences in his success:

(*a*) While the particular kind of subject-matter which he

takes up will depend mainly upon the direction of his interests and opportunities, he is unlikely to achieve very much unless his preparation takes him into potentially overlapping fields of scientific exploration.

(*b*) It is likely that the most critical early steps that he takes will be due to prompting coming from people, or from other sources, outside himself and his own planned beginnings of experiment.

(*c*) It is almost certain that as his work develops he will follow a great many more clues than he eventually uses. He must know when to stop trying a particular direction of experiment, and he must not be afraid to do this.

(*d*) Experimenting and interpreting the results of experiment are not normally processes laid out in a strict succession; they go on together. Usually, however, there are final steps specially directed to such interpretation and presentation of results as will display order and system in a mass of detail. It is not uncommon for the individual experimenter to get very much held up at this stage and he may have to put his own results out of mind for a time. When this happens it may be because, once again, the most illuminating interpretations often come from outside the immediate experimental range.

(*e*) An experimenter must "know" what dimensions of change it is likely to be most worth his while to select for experimental control. This seems to demand some "superior sensitivity" to "dead ends"—if the experiments get more or less near to them—and to the proximity of openings so many, so varied, and so general that if he proceeds he is likely to wander aimlessly.

(*f*) Prediction, as the experimenter uses it, can either take the form of pointing out the most likely lines for experimental development before they have been embarked upon, or of stating expected results from novel experiments on the assumption that certain general principles have been correctly formulated. The second form of prediction is always to some degree deductive and so it has some common characters with formal "closed system" thinking. It may or may not involve probability calculations.

(*g*) Science in its advance moves towards measurement. The

quantification possible in a great many instances, especially in biological fields, is statistical, and since at present in such cases the values assignable have to be determined by experiment, any experimenter working in these fields now needs enough statistical background to select and use appropriate methods, if he is to think effectively.

CHAPTER NINE

Adventurous Thinking—4

EVERYDAY THINKING

1. THE SCOPE AND THE METHODS OF THE PRESENT STUDY

By everyday thinking I mean those activities by which most people, when they are not making any particular attempt to be logical or scientific, try to fill up gaps in information available to them in which, for some reason, they are specially interested. The gaps are of the kind that cannot be filled entirely by number or form sequences, or by words which must exactly fit some predetermined structure—as in crossword puzzles, and acrostics. They have to be filled in a descriptive manner, usually by word sequences in which the word items themselves are employed with very great freedom, constrained by little except the size and contents of the thinker's vocabulary, various aspects in the social occasion of their use, and the fact that all the sequences must appear as interconnected and as filling the gap in a manner which the thinker hopes will be approved and accepted.

Other signs besides words can be used in everyday thinking; gestures, for example, and, more rarely and with more limited range, colours and tones. But then they must have the same immediate communication functions as words can have; for everyday thinking can also be called "immediate communication thinking." It can find expression in speech, or in some kind of miming, or it can be written. If it is written it has neither the purpose nor the character of the permanent record, but is of the letter style, or that of the newspaper intended only for the day, or, very occasionally perhaps, that of a genuinely private journal.

As we have seen already, if knowledge is to become

increasingly exact, in any department of human interest, experiment must grow out of earlier natural observation. Early experimental results will usually lead on to more specific and defined natural observations, and from these again will develop further steps of experiment with improved control; and so this kind of process may go on until it seems that the limit of control for the kind of subject-matter concerned has been reached.

The sorts of observations which may perhaps be used to suggest experiments about everyday thinking are within the range of almost everybody. We cannot help overhearing conversations in bus, train, and on the street. Any day we like we can read the speeches of politicians, which are generally at least intended to be cast into a popular form, and are rarely or never in the precise "closed system" or "experimental" style. Nowadays all wireless and television programmes contain frequent items which are specifically shaped to present and to stimulate popular thinking. Everybody, except the most complete recluse, belongs to various social groups and takes some part in their goings on, and as he does this he must have many opportunities to study the predominant topics and style of immediate communication.

Observations in these directions and, of course, in many others that might just as well be selected, can provide data and suggestions for experiments; for it is certainly possible, to some degree at any rate, to design and do experiments upon everyday thinking. Narrative, or other kinds of material, dealing with topics and in the style which the observations have shown to be most likely to make people want to talk, or express themselves in some other way, can be produced, but in various respects left incomplete. It can be written for a reader, or recorded for a listener, who can be invited and encouraged to try to fill up the gaps, maybe in writing, maybe in speech, but always in an informal manner. Again, it is not difficult to start up "free" conversations and discussions, and then to throw in predetermined interjections at intervals, and to have the whole tape-recorded, for subsequent analysis. By making these and like trials, sometimes with the single person, and sometimes with groups of people, it is possible to get at least

certain pointers to ways in which social influences may affect popular thinking. Without question experiments of this kind are elementary and qualitative; but in these respects they are no worse than many other early attempts at experiment in directions which have proved amenable to much development. At any rate I hope I can show that they are better than no experiments at all.

The range of themes and the variability of approach and treatment which all find their places in everyday thinking are so vast that the present brief discussion is bound to appear scrappy and selected. However, since my main hope is to open up the subject for further, better, and more comprehensive investigation, scrappiness may be in this case a positive virtue.

The observations and experiments upon which the bulk of this discussion is based were developed in the middle 1930's and have been continued—though only very intermittently during the war period—ever since.[1] All I can attempt is to select a few illustrations, and to put forward in a general way, and without detailed evidence, certain conclusions which may help to establish some important relations, of agreement and of difference, between the tactics and aim of everyday thinking and those characteristic of thinking in the closed system, and in experiment.

2. EXPERIMENTS WITH A SOCIAL THEME

Anybody who accumulates evidence from the observational sources already mentioned will speedily find that the overwhelming majority of the situations which set people talking and speculating in their everyday lives, have a strongly social

[1] Although a great quantity of evidence has been collected, both by myself and by other Cambridge students, very little of it has so far been published. I wish to thank Dr. Frisby, Director of the National Institute of Industrial Psychology, for permission to use a paper entitled *The Co-operation of Social Groups* which I published in Occupational Psychology, Vol. XII, 1938, pp. 30–42. There are three valuable papers by D. M. Carmichael in Brit. Journ. Psychol., XXIX, 1938, pp. 206, 329, and Brit. Journ. Psychol., XXX, 1940, p. 295. It was a part of the original programme to try to complete a number of comparative experimental studies in different social communities. Carmichael's West Greenland experiments were made public but unfortunately a great bulk of other material, collected in India, Africa, and in most of the nation groups of Middle Europe, was lost in the war.

character. They may, and in fact most of them do, directly concern personal relations, but also they often deal with the relations of different social groups. Even the conversations and disputes which spring from what may at first seem to be a wholly objective outlook—closing times for shops; the "lateness" or "earliness" of seasonal changes, school examinations—rarely continue for long without getting a social and personal twist.

There is another striking feature of immediate communication thinking which cannot fail to be noticed. This is that while the basic themes which set people thinking in everyday life remain fairly constant from group to group, and from one period of time to another, both the particular topics and the forms of expression adopted are strongly influenced by current affairs and fashions.

(*a*) *On the Co-operation of Differently Organized Social Groups*

About the time when I began to try to develop some of these thinking experiments, there were few problems of contemporary social life that were attracting more discussion than those connected with the co-operation of differently organized social groups. At the time there was a strong tendency to look at such problems in a negative way, to see them mainly as problems of how to remove social tensions. This must have been largely due to a strong influence of popular forms of Freudian psychology, which saw all forms of conflict as due to repressions, and took the easy view that to bring to light these repressions and their conditions would itself put an end to social dissensions. However, there were already plenty of people in the period between the two wars who were thinking in a more positive manner about questions of group interrelationship. They could see that modern developments of science and of popular education had already made it impossible that any effective social group anywhere should live its life alone, and that unless friendly relations could be widely and firmly established between groups which may be differently organized, may possess a different membership, different aims, and different traditions, civilization might disappear in confusion and disaster.

During the same period an enormous amount of public effort and expenditure were devoted to devising, discussing, and even providing machinery which it was expected would secure widespread social and group co-operation. As everybody now knows, much of this machinery was of a highly elaborate kind, and took legal, economic, diplomatic, political, administrative, and executive forms. Yet at the same time, hardly any notice was taken of the ways in which the bulk of the people of any group were thinking and talking about such problems.

No doubt it is debatable whether what most of us may think about interesting social movements which are in course of development has any great effect upon the way in which those movements do in fact develop; but, apart altogether from that, it seemed sensible, in the middle 1930's, to start experiments on popular thinking by presenting unfinished accounts of various attempts to bring about co-operation between different social groups.

Accordingly, I devised a number of concrete situations, all involving group relations not too far removed from the possible experience of the people who would deal with them. They were left in an incomplete state, and the subjects in the experiments were to be asked to continue them to what they thought would be their most likely issue.

Here are two of the many instances which I and other students found of interest.

A. EXTRACT FROM A LEADING ARTICLE

Yesterday the culminating step was taken in a series of negotiations which, as most people know, have been steadily progressing for several months. Mr. Ardern, representing the Union of Agricultural Engineers and Mr. T. Smith, of the Amalgamated Association of Agricultural Labourers, set their signatures to Articles of Agreement which, it is hoped, will bring these powerful but widely different groups into close and fruitful co-operation. In many quarters it has for long been maintained that the different organized sections of any complicated industry must be brought to unite if the best interests of each section are to be secured. It is undeniable that in the past the increasing invasion of agriculture by the skilled technical worker has produced a great amount of irritating friction. In the mass, the agricultural

labourers of this country represent a solid but excessively conservative element of the community. The alert engineer, on the other hand, marches in the van of an incalculably rapid and bewilderingly varied progress, and is perhaps apt, on that account, to be of a radical frame of mind which welcomes change, and tends to regard attachment to ancient methods and practices as timid and bovine.

A survey of the Articles of Agreement makes it clear that one of the outstanding hopes of those who have worked for this union of groups is that thereby the bargaining powers of the labouring class in agriculture, towards securing for themselves better economic conditions, will be enormously increased. Indeed, a superficial study of the Articles leaves one with the impression that the Association of Agricultural Labourers have far more to gain from the proposed co-operation than have the Union of Agricultural Engineers. But deeper considerations make it obvious that no such impressions can be long maintained. Ultimately, the interests of all sections of any organized industry must be congruent, and any amelioration of the conditions of any one section must work for the betterment of all. This fact, however, is frequently obscured, and the achievement of this desirable end is hindered by sectional jealousy and the biased demands of short-sighted agitators. It is a welcome sign of the times that the Agricultural Engineers clearly recognize that their own good fortune must be founded upon the bedrock principles of justice and cannot be finally and firmly secured save by active concern for the lot of their less fortunate brothers in industry.

Already critics are not lacking who endeavour to point out that these Articles contain the seeds of division. They say, with undoubted truth, that the mode of organization of the Engineers differs greatly from that of the Labourers. The former, with their vastly greater internal differentiation, have developed along lines which allow far greater scope and power to the expression of local opinion. Their leaders are democratically elected and are, in a true sense as regards their policy, the servants of those whom they lead. The latter, varying far less among themselves, and from district to district, have never developed local organization to a high degree. The Labourer leaders have come to the front mainly on a basis of their particular personal qualities, have framed their own policy and maintained it, when they have been successful, primarily because they possess the power to persuade or to command. It is maintained—perhaps we should say rather it is hoped—that co-operation between groups whose organization is on the one hand democratic and on the other despotic can never succeed.

Opponents of the Agreement have even sought the aid of "science."

They have called in the ubiquitous psychologist. Apparently some eminent and ingenious person, armed with methods which it is not easy to understand, has "demonstrated" that the "average I.Q." level in the Union of Agricultural Engineers is 160, whereas the corresponding measure in the case of the Amalgamated Association of Agricultural Labourers is 90. From these mystic figures dire results are prophesied.

The essential rightness of the Agreement remains. We are convinced that we speak for the solid mass of public opinion in this country when we assert that its principles will stand firm against all selfish and narrow-minded attack, an object-lesson of the way in which industrial organization should and must move, not only in this country but throughout the world.

B. STRANGE EFFECTS OF A DROUGHT

A most peculiar consequence of the prolonged drought of a recent spring and summer has been observed in a village of North Western England. This village, which is somewhat widely scattered over a large parish, and consists in all of some 1,500 inhabitants, has been disturbed by religious differences for many years past. From time to time the strife between the factions represented has been extremely acute. What may, for the purposes of this description, be called the indigenous element of the population of this district contained a strong mixture of Roman Catholics. Although the partisans of this group long ago moved away from their professed adherence to the Pope, they have always retained, to a large extent, the ritualistic outlook and beliefs, and the Established Church in this village has, for hundreds of years, so ordered its practices as to bring them as nearly into line with Roman Catholicism as the general religious trend of the Church of England would allow.

About the time of the Industrial Revolution an attempt was made to establish an industry in this area. Although the attempt proved abortive, it brought into the district a number of immigrants from villages and towns in Wales and the Midlands of England, and a certain number of these stayed on after the failure of the industry and occupied themselves in the regular agricultural pursuits of the community and in petty shopkeeping. A few even succeeded in acquiring a little land and became small-holders or farmers. These, however, in the main, had a strong Nonconformist outlook, which tended to become accentuated by contrast with the High Church practices of their new neighbourhood. Thus there grew up a feud, which each side passed on to the children. Not only in their religion,

but in the practices of their daily life, these two groups lived side by side, but remained separate. They bought and sold in different quarters; their social amenities, their athletic clubs were separately organized; and if ever they met in contest the battles were bitter to a degree. Very rarely indeed was there any inter-marriage from group to group, and whenever there was, the contracting parties were invariably driven to seek a new neighbourhood. Each side was a fertile source of discreditable story and rumour about the other. On special occasions, as during a fairly recent proposal for the revision of the Prayer Book, antagonism flared up into actual fighting. In spite of the smallness of the population, this was one of the villages where a church school was still maintained for the children of the one party, while a public elementary school was provided for the children of the other.

Upon this divided community fell a tremendous and lasting drought. The water supply, as in many other places, was totally inadequate for the emergency. The condition of the village threatened to become deplorable. There was indeed one inexhaustible and bountiful well. But it was in private ownership. The local landowner, upon whose estate the well was to be found, had but recently acquired his possessions in this neighbourhood. He had bought them from the ancient but decayed family which had held them for generations, and had been one of the main pillars of the Church party in the village. He had, in fact, practically no local interests, and was an absentee owner, except during the grouse-shooting season in the autumn.

Both parties approached him with a request that they might, in their distress, use his well. Whether as a result of a peculiar sense of humour, or of a desire for social experimentation, or, as is perhaps most likely, out of mere ignorance, he acceded to both requests, but laid it down that water must be drawn only during certain limited hours of the day. Consequently members of the two parties began regularly to be thrown into unusually close conjunction as they hurried to secure their daily supplies of water. While one person was getting his share, others, perforce, had to wait. It might have been expected that party rivalry would have become acute under these circumstances and strife exacerbated. But even from the beginning, and less and less as time went on, this effect was not to be discovered. Since the hours of the drawing of water were limited, but all the ordinary processes of labour must continue as usual, it soon became the custom for one family to draw for another. Also, since the distance was considerable, and the weak and ailing could hardly draw for themselves, others willingly stepped in and helped them out of their difficulty. The common need seemed to override traditional hostilities.

People were thrown together in ways new to them, and as the long drought continued, a novel spirit of friendliness seemed to spread throughout the village.

The rainless summer wore slowly on and the day of the annual village flower show approached. From time immemorial this show had been held, and the village was determined that it should continue. As long as ever anybody could remember, this occasion had been one on which jealousy, hatred, and hostility had threatened to pass all bounds. But now the show had become more a matter of maintaining the honour of the village than of enhancing the prestige of any particular group of cultivators. And so the water which had kept flowers and vegetables alive had been contributed by all from their scanty supplies to a common stock, and from that stock drawn with scrupulous care and respect for the needs of others. In the luncheon tent, on the day of the show, the Rector sat next to the Baptist minister, and Sadd, the churchwarden, was side by side with Poper, the local preacher. In his speech the Rector remarked that recent events had taught them all that differences of religious practice were but the varied expression of a fundamental identity of need, and though hardly any of his audience could understand his words, everybody vigorously applauded his sentiments. He thereupon publicly shook hands with the Baptist minister, a thing unprecedented in the annals of the village. Mr. Poper proposed, and Mr. Sadd seconded, that bygones should be bygones and that henceforth and for ever the village, of which all of them were proud, should live as one friendly group. This was carried with enormous enthusiasm.

A cynical and observant onlooker might have shrugged his shoulders, but had he been less cynical and more observant he would, perhaps, have noted that, as the acclamations rent the air, the glances of Miss Salversol, the headmistress of the Church school, and of Mr. Slize, the headmaster of the public elementary school, met and that each smiled to each. During the summer Miss Salversol and Mr. Slize had met, first by the well, but later in many other places. Inspired by a common professional interest each had confessed to the other that neither school could be regarded as ideal. After the flower show they went away for their holidays. They returned as Mr. and Mrs. Slize, and though a few scattered rains had already begun to appear, they were by no means treated as they would have been less than a year before, but settled happily to their former tasks. The latest information shows that a new and more commodious school building is to be erected in the village. It is anticipated that Mrs. Slize will continue to give religious instruction to certain children and Mr. Slize to others. But for all secular purposes the school will be treated

as a single unit, and the peculiar excellences of Mr. and Mrs. Slize will combine to impart an education at once modern and, it is thought, complete.

This material was given to some subjects as individuals and to others in groups. In the former case the description of the situation was either typed or recorded. If it was typed a subject read it for himself and could refer to it in any way he pleased while he dealt with it. If it was recorded he could hear all or part of it as often as he desired. When groups of subjects worked together they were given recordings.

With instance A the instructions were these:

Assuming that the *facts* are as reported, and as far as you can, ignoring whether the *arguments* stated are good ones or bad ones from a logical point of view, try to think whether the co-operation sought between these two groups is likely to be maintained successfully or must inevitably break down. State also the steps by which you think you arrive at your conclusions; but don't argue about them.

With instance B the instructions ran:

Considering only the *facts* as reported and without regard to any opinions that may seem to be expressed, try to estimate the chances that the co-operation introduced into the village under the special circumstances will be lasting. Also, if you can, state the main considerations through which you have arrived at your decision.

It may have been a mistake to avoid any logical assessment of the material as described, and to rule out any argument about the decisions as reached. Maybe, and perhaps for the same reasons, that as exact thinking hankers after the closed system, so popular thinking hankers after the argumentative form.

(i) *Arriving at Decisions.*—One way or another these two instances, and many others of more or less like character, have been given to a very large number of people, taken at random as opportunity offered. As a rule they have aroused immediate interest and response, and often they have stimulated lively discussions. It would certainly have been worth while to make tape recordings of some of these; but I did not do that. I have

found it rare to meet people who will not venture on definite opinions about the issues raised and when this has occurred it has, without exception, been with the individual subject—never with the person who is responding as a group member. It happens only with a person who tries to take into account at the same time all of the various points of evidence offered, and my strong impression has been that they are people who have adopted this general attitude towards whole classes of situation, or perhaps towards life in general, long before they took part in the experiments. That is to say the lack of decision is not to any great extent due to indeterminateness in the material, though it is sure to be ascribed to that; it is temperamental.

About four people in any hundred refused to decide anything about the probable future in case A, and rather fewer than this in case B; but, of course, there is nothing final or necessary about these proportions.[1] It is quite certain that everyday thinking is strongly slanted towards definite decisions. Further, all the results that I have collected agree in indicating that the way these decisions are effected and justified is by a selection of part only of the evidence that is available. It is perhaps inevitable that in these two instances, where the issue is a straightforward "permanence-impermanence," assigned evidence for declared issues is more variable than the decisions declared. But in other instances, where the number of possible issues is increased, it remains true that items of evidence assigned in support of the issues are still more varied.

The truth is that in everyday thinking any person enters the circumstances which set his mind to work already predisposed in favour of certain argument sequences, and against others. When we were considering thinking in the closed system we found that generally, and perhaps always, the number of empirically probable next steps at any stage of evidence is less than the number of theoretically possible next steps. The discrepancy between amount (number of items) of evidence that could be used, and amount that is in fact used is very much greater in popular thinking than it is in closed systems or in

[1] However, D. M. Carmichael, who used instance A in an independent study in Scotland, got very nearly the same distribution. See Brit. Journ. Psychol., XXIX, 1939, p. 332.

experiment. The psychological determination of this is that in popular thinking the end of the preferred argument sequence itself takes charge of the selection of particular items of evidence, which are then used as if they set up the sequence leading to the issue accepted; but in the closed system it is the objectively necessary system structure which produces a genuine advance through steps to issue, and in experimental thinking it is as nearly as possible a dispassionate survey of the evidence which decides the order of steps and through this the issue. The tactics of popular thinking are thus markedly different from those of the other kinds. Presently we shall see that its strategy may be different also.

(ii) *The Use of Evidence.*—With instance A, and considering my own overall results, there are nearly twice as many people on the side of breakdown as on the side of permanence. All told, between 60 per cent and 70 per cent favoured breakdown. The total group could be divided broadly into academic and non-academic, most of the latter being weekly wage-earners. Four-fifths of the practical (non-academic) group favoured breakdown, a good many more, that is, than in the academic group. In this and in most of the other instances, the frequency of occurrence of preferred issues varied with the constitution of the group, although the issues preferred were extremely likely to remain the same for all groups. Generally speaking, where political and economic evidence could be used, the more "practical" type of group consistently tended to be more often in favour of negative issues.

With case B, the overall results showed a large majority of about 8 to 1 in favour of lasting co-operation. There was no important difference between academic and practical groups in this case. These results were somewhat surprising to me, because I had been prepared to assume that economic overlap would be regarded in a popular sense as a powerful influence for social co-operation, and religious differences as a powerful influence against social co-operation.

From our present point of view, it is more important to consider how the actual available evidence was alleged to have been used as leading to the decision favoured. In case A just about a third of those who thought breakdown would occur

picked out the difference of intelligence as inevitably leading to this. In the practical group the difference appeared still more decisive and more antagonistic to continued co-operation than it did in the academic group. About a quarter of all the subjects considered that breakdown must occur "because" one group was democratically led and the other was not. Another quarter went outside the evidence given, asserting that breakdown must occur on account of the general conservatism of agricultural labourers, and the remainder went farther afield still, and set up a sequence of their own; attempted agreement will lead to an increasing use of machinery, a consequent displacement of unskilled labour, and an inevitable breakdown.

In case B, nearly one-third decided that the establishment of a common educational system would ensure continuing co-operation. About a fifth said that with already weakening antipathies, any "occasion" would lead to their final disappearance. Another fifth simply asserted in general that religious differences nowadays are superficial. Nearly as many decided that the friendship of the group leaders would maintain co-operation. The rest declared that co-operation would be more "pleasant" in the general life of the village and so it would continue.

Of the minority, in case A rather more than half decided quite generally that overlap of economic interests would defeat all tendencies to disruption, and the rest pointed to the "fact" that the engineer leaders would be in a dominant position, and that this would swing the labourers towards a more democratic organization. In case B everybody alleged that he based his decision on the deep-seated, permanent character of religious differences.

If we examine the results in these two instances we find:

(1) The same decisions are alleged to be reached now from one specific item of evidence, and again from a different specific item. It seems that the decisions are not so much reached through the evidence as that the evidence is picked out in accordance with some decision already made.

(2) Sometimes the evidence alleged is not in the evidence given, but is treated as consistent with the evidence given, and there is a suggestion (it was borne out in other cases) that this

is the more likely to be the case as the issue accepted is unfavourable, unpleasant, and opposed to social concord.

(3) Normally the evidence actually used, or at any rate advanced, in favour of the issue accepted, falls short of the evidence that might be used. It is usual for the decision as to the issue to be announced first, and then for a single head of evidence to be advanced as the alleged basis for the decision. Results tend to indicate that minority decisions are more likely than others to be announced in some entirely general form, and for no specific evidence to be advanced.

(iii) *Experts and Evidence.*—In the two instances so far considered none of the subjects who pronounced decisions could be regarded as particular experts. D. M. Carmichael, however, used for one of his instances a leading article from *The Times* newspaper (July 1937) which was concerned with a possible increasing co-operation between A.R.A. (Amateur Rowing Association) and N.A.R.A. (National Amateur Rowing Association) in this country. Some of his subjects were rowing men and some were not, and the former can be regarded as an expert group. Of the experts, 60 per cent decided that the upshot of attempted social co-operation was "doubtful." Of the rest only 16 per cent arrived at this decision.

Nobody is likely to be surprised at such a result, but what is of considerable psychological interest is the difference of procedure of the non-experts and the experts. The former almost invariably rested the superior certainty of their views upon conventional generalizations about group relationships which they had already accepted (e.g. co-operation will continue—a common interest in sport eliminates class-distinction; or, breakdown will occur—two groups officially combined from the top, will preserve their internal rank-and-file rivalries). They made no great use of the evidence as presented in the leading article. The experts, on the other hand, brought forward many points of evidence, but most of these were drawn from their own personal knowledge and experience and not from the case as put before them by the experimenter.

Altogether it seems that the part played by impersonally presented evidence in shaping everyday thinking about group contacts and relations can be easily overrated. Even when

evidence as assigned is used it is extremely rare for all of it to be used. As more and more of it is used, especially as supplemented by direct and specialized information, it becomes the less likely that definite issues will be reached.

(iv) *Thinking in Company*.—Already it has appeared that everyday thinking about such matters as the relations of social groups, consists in the main of some single generalization, advanced as if it were incontestable, with or without evidence; but if with evidence, usually with less than might be used. Generalization and selected evidence are alike strongly socially determined. The first can nearly always be found to be current in some group of which the thinker is a member, and the second, provided it is not just personal recall, is precisely the same evidence that many other members of the group also select.

When the experiments were carried out in company, whether or not there was open communication between the different subjects, it nearly always seemed as if there was a further direct social influence at work. This favoured negative issues (breakdown rather than continued agreement); disparaging items of evidence, and destructive developments. More and better experiments are needed to prove this point, but it is likely to surprise nobody that people in company who are thinking in the manner of every day, and without much deliberate care, are negative rather than positive, destructive rather than constructive.

(*b*) *On Other Issues having a Social Setting*

I, and various students, have also made use of many other descriptions of situations involving personal rather than group relationships, but remaining in some respect indeterminate and incomplete. Several instances are given in D. M. Carmichael's article entitled *Examples of Constructive Thinking amongst Greenlanders*,[1] and one of my own is reproduced in full in *The Mind at Work and Play.*[2] All of them were put into a concrete, narrative form, for this form produces both the most decisive and the least artificial responses.

The sorts of events depicted were, for example:

Friends discuss together what they would do should certain

[1] Brit. Journ. Psychol., XXX, 1940, p. 295. [2] Chapter VI, pp. 134–5.

unanticipated circumstances arise, and agree upon a course of action as the most fitting one. Shortly afterwards the unlikely situation in fact happens. How will they behave?

Some Government department declares its intention of taking over a stretch of country near a village and making a weapons-range. A leading person in the village organizes opposition to the scheme with practically unanimous support. The day after the main protest meeting all the daily newspapers contain an announcement. Later the range is opened, with enthusiastic backing from everybody in the village, led by the original principal opponent. What was the nature of the announcement that was made, and how did it affect personal relations in the village?

A set of people are discussing the broken career of a man they all know. Each person proposes something that the man might do; all of the suggestions imply that whatever he does he will lose "caste." What was it that broke up his career? What are the probable future relations of the members of the set to the man they are discussing?

(i) *Specification and Generalization.*—So far as general procedure goes in dealing with instances of this kind, all the characteristic features already mentioned recur. An impartial, or even an all-inclusive, survey of the available specific data is exceedingly rare. By far the most likely thing to happen is that a little of the evidence supplied is accepted and the rest is ignored or distorted. Some generalization, usually not one that is present in the description itself, but one taken directly from the accepted social conventions of the group to which the subject belongs, is used to interpret the partial evidence that has been accepted. It is frequently difficult to tell whether the generalization determines the items of evidence used, or whether the items of evidence are immediately "dominant" and themselves lead to the generalization. It seems, however, to be fairly safe to say that when the case dealt with is one that can be treated as if it belonged to the thinker's own group, the "key" item procedure is the more likely to be adopted; but in proportion as the case is "removed" (far away in space, or time, or unfamiliar) the generalization comes first and settles what use, if any, is to be made of specific evidence.

No important differences of procedure have ever turned up as between groups vocationally, or in any other way, different in the same community, or between groups in widely separated communities. Naturally and of necessity the generalizations and the related specifications that are preferred vary widely from one group to another, but the procedure remains the same.

There is one sort of specification, never at all present in the same way in closed systems or experimental thinking, which is very common indeed in everyday thought activity. This is the resort to the ego. The more general the situation left incomplete can be made to appear, the more likely is reminiscence to come in: "Of course a Government department will choose the most unsuitable area for a weapons-range. Two years ago, when I was on holiday with my wife in . . ." In a collection of correspondence about issues of popular interest presented in the same manner as the ones used in these experiments, two out of every three letters took the form apparently of deciding a question by a single personal memory.

It seemed as if there was sense in saying that experimental thinking by its nature likes agreement, overlap, and analogy. Perhaps everyday thinking finds it particularly easy and pleasant to jump from one special case, of the thinker's own, to another. It is also possible that this is preferred because the thinker is all the time operating with generalizations which have for him passed beyond any stage for criticism, and need not even be formulated.

(ii) *No Steps?*—We have seen that the analytical thinker operating in a closed system can, and generally does, set out his processes of interpolation, extrapolation, and reconstruction into a series of necessary steps. And the experimental scientist can, and does, set forth his growing knowledge of the structures and principles of the systems in which he is working in a step series, each successive member of which can be shown to follow in a necessary way from preceding members. The experiments indicate that in everyday thinking no such step series is normally present or required. Once the selected, or weighted evidence, and the accepted generalization are brought together (or, in some instances, once the accepted generalization is simply applied to the case in hand) the required continuation or com-

pletion of the situation that has provided the occasion to think, is simply "there." There are no traces of elaborate processes still interposed between the data for completion and the completion itself. The completion is accepted and asserted with conviction, is "fitting," and no more can be said about this than can be said in the more elementary instance in which a tactual pattern is treated as a "match" or a "fit" with a visual pattern.

This is not, of course, for us any new constituent of thinking. We saw that when a closed system possesses a very elaborate structure one thinker may appear to differ from another largely in his sensitivity to the prospective "fitness" of a particular step. We saw also that the original experimenter very often seems to be the one who is sensitive beforehand to that direction of trial which will best "fit" the requirements of his developing research.

Everyday thinking is not peculiar in claiming a capacity to detect "fitness" and "match." The difference is that in the other two cases the fitness can be demonstrated, and generally, though not always, by the thinker who detects it. In everyday thinking it usually cannot be demonstrated at all, or if it can this almost invariably takes a very long time.

This is where we can begin to see clearly a genuine difference of strategy between everyday thinking and the other kinds. The closed system thinker and the experimental scientist are set to achieve results which carry with them an internal compulsion. Everyday thinking also aims to arrive at a compelling issue, and, however it makes its journey, to stop where every other normal person must stop. But the steps to the resting-place are never all of them exhibited, and there is no inherent compulsion in their nature and relations. It is not in the nature of the assertion, but in the manner of assertiveness that everyday thinking seeks to attain necessity. Its commonest introductory phrases, when it is expressed in words, are "of course," "beyond a doubt," and—especially perhaps in political circles—"I am (or "we are") confident that." The source of the compulsion being now within the thinker, and particularly in his social group, it is possible, and indeed common, for completely contradictory issues to claim the same necessity. Then the only way either side has of enforcing its claim is yet more

violent assertiveness. It is partly on account of this that many people have said that everyday thinking is largely emotional thinking. Perhaps it would be more accurate to say that when the results of everyday thinking are challenged this is exceedingly apt to lead to emotional display in a way that can occur with no other form of thinking so far considered.

3. SOME FURTHER FEATURES OF EVERYDAY THINKING

(*a*) *Generalizing*

In view of the great part which generalization plays in everyday thinking, this is as convenient a place as any to consider the character and conditions of the generalizing processes. Experiments and observations that anybody can make will convince him that the number and sweep of the generalizations used in everyday thinking are such that no ordinary person could possibly arrive at them by a gradual process of abstracting common features from a lot of different settings. The great majority of these generalizations are, in fact, taken over ready made, by the thinker, from the society into which he is born. Usually there are many more of these in common use than are needed on any particular occasion, and so the thinker must select from among them those that best fit his immediate aims. Selection may be impartial or biased. Impartial selection is a slow process, requiring the examination by comparison of a number of possibilities; and a study of everyday thinking reveals little or nothing of this technique. The thinker, approaching the situations which provoke him, is already predisposed in favour of some conventional comments and against others. Often he appears to adopt as his own, without any further examination, the first conventional generalization current in an approved social group, which "comes to mind," as we say. But "comes to mind" is not a chance affair at all. In this case, as in many others, it is a joint result of what the mind wants, and what the circumstances offer, with the former playing a dominating role.

Much of what is called generalizing, then, as it is found in everyday thinking, is no more than the acceptance, with biased selection, of already formed social conventions; but there may

be more to it than this. One thing that is very common is formula making by model. When a lot of social conventions are established and available, all of which when they are formulated have that sort of sweeping range which can cover all manner of particular occasions, it becomes easy for anybody to treat them as models, and so to cast the apparent results of his own single and limited experiences into the same sweeping form.

Perhaps, however, generalizing has to have a psychological beginning somewhere, and although, as it occurs in everyday thinking, it always involves some kind of social setting, we should not suppose that it is nothing more than accepting established conventions or treating them as models. Here we must remind ourselves that everyday thinking can just as well be called "immediate communication thinking." Immediate, here, means dealing with highly topical events when other persons are present or anyway near. The communications are then likely to share the urgency of the events, and this to favour a strong and unconditioned mode of expression. The same mode is still further strengthened by all those self-regarding sentiments from which nobody who enters into the "immediate communication" situation can escape. Even supposing there are no ready-made social conventions available, immediate communication thinking soon adopts the sweeping, generalized form, and does not do so by a painstaking, slow, and intelligent comparison of many instances, and the abstraction of common features, but by an easy and natural exaggeration which in the extreme may be based upon one single impressive personal experience.

It should now be clear that the generalizing of everyday thinking is very different in its psychological character from that which we discussed in connexion with closed system thinking,[1] and also from that which was implied in our considerations of experimental thinking.[2] What they have in common is that they can all spring from only a few particular instances. Where formal or experimental structures are concerned, however, the instance, or the few instances, are patiently explored for whatever "rules" they may reveal. In everyday

[1] See pp. 92–6. [2] See pp. 148–51.

thinking the instance, or the few instances, are not so much explored as, so to speak, "savoured" and used. The generalizations of the non-exploratory kind have little to do with transfer of practice or training save that they make it more difficult.

(*b*) *Timing*

When any process is used for communication, timing problems are bound to become important. Verbatim records of more or less impromptu discussions will show, for example, that these usually take an even more wandering course than that of the prolonged experimental investigation; and also that it often happens that contributions come in at one stage and elicit no response, and then, in precisely the same form, come in at another stage and at once dominate the whole subsequent course of discussion. The biographies of men of affairs are very nearly as full of references to timing as are the writings of sports commentators. W. E. Gladstone, for example, "considered that his sense of timing was his most valuable political gift. He noted at the end of his life that 'the most striking gift . . . entrusted to me . . . is an insight into the facts of particular eras and their relation to one another, which generates in the public mind a conviction that the materials exist for the formation of a public opinion and for directing it to a particular end'."[1]

If we take the view, as I have done, that everyday thinking is essentially communicative, it seems that we must agree that its successful practitioners are especially sensitive to the timing of their communications. This is perhaps the most important form which response to direction takes in the kind of thinking process that we are now considering. Very possibly the special sensitivity on which it is based is simple and direct in itself; but it is exercised upon data which are usually extremely complicated. It seems to be a rare acquisition, claimed far more frequently than it is possessed. It must differ in technique from sensitivity to regularity of structure, or to the most likely line of effective experimental advance; but all these do seem to have in common that they are responding to converging evidence, maybe items

[1] *Gladstone, A Biography*, by Philip Magnus (London: John Murray, 1954, p. 190).

that are coming together into a kind of architecture; maybe lines of approach that are advancing to unite in a new discovery; maybe streams of opinion, fashion, whims which will some day, not far off, come together to welcome a communicated policy. People who seem to possess this sensitivity are called "wise" by their contemporaries, and if their timing proved to be just and right, also by posterity. "Wisdom" is a much more appropriate word than "instinct," if only because in whatever field "timing" is exercised, it demands width of knowledge and accumulation of experience. But what is the constitution of wisdom, and whether there can be any certain way of building it up, still wait for answers.

(c) *Point of no return*

When I look into my collection of recorded discussions, and reflect upon my observations in committees, and especially as a chairman of committees, I come constantly upon instances of how people start developing an argument sequence, perhaps rather tentatively, and reach a stage at which it is very much easier to go on than to go back. A little beyond this there comes a stage when, if they do make an effort to go back, they become hesitant, ineffective, and very often incoherent. They have reached a phase which, when we were considering the leading characters of bodily skill, we called the "point of no return."

Once again, however, there is a difference between point of no return in closed systems, or in experiment, and in everyday thinking. And the difference is again largely due to the fact that everyday thinking is immediate communication thinking. What compels the formal and the experimental thinker to go on is, so to speak, the prestige properties of the sequence upon which they have embarked. What prevents the everyday thinker from returning is largely his personal prestige in the communication situation. So there is much more diversity between points of no return for the latter than for the former.

These points of no return can be found, not only in the everyday and popular thinking of the individual, but also in the development of public opinion, fashion, policy, and practice in

many different directions. It may at present be no more than a guess, but it seems to me very likely that, as a social group becomes larger, so does it tend to reach points of no return more quickly, and so, even more markedly, does it become harder and harder to reverse thinking and policy which have been carried to this critical stage.

(*d*) *Compromise*

There may seem to be something paradoxical in raising the question of compromise immediately after pointing out that everyday thinking is apt to be expressed with great vigour and decisiveness and, once it has reached a certain phase, is particularly reluctant, and even unable, to retreat. In fact, however, as regards issues there is no compromise for the thinker in the closed system and none for the experimental scientist; only for the everyday thinker. The scientist may, for various reasons, have to put up with methods of research that are only approximately the ones that he would use if he had a perfectly free choice. Also both the closed system thinker and the scientist may treat these issues as probable only, but then always with the implication that the limits of probability can be exactly stated.

Indeed, all these four features of everyday thinking: generalizing, timing, point of no return, and now compromise, arise directly from its social basis and functions. Because an issue is expressed in an exaggerated manner, at a time when it is likely to have a resounding effect, and, having reached a certain stage, must continue, or run a great risk of failure, when issues are opposed in immediate communication the only solutions possible are a downright fight or a compromise. Everybody knows that it is exceedingly rare, if not completely impossible, for the immediate antagonists in such a situation to reach a compromise. This requires a third party who must try to use everyday thinking to get out of the difficulty which other everyday thinking has produced. To consider in detail how this can be done is no doubt a matter of interest and importance, but it would take us too far afield.

CHAPTER TEN

Adventurous Thinking—5

THE ARTIST'S THINKING

1. A DIFFERENT KIND OF THINKING

Nobody is likely to deny that the artist's thinking is different from the kinds we have considered up to now. There have, indeed, been persons who apparently wish to maintain that certain sorts of artists, especially those whose proper medium is colour, shape, form, and tone—painters and musicians—do not think at all. But this question need not worry us, since whatever else they may do, artists are all the time endeavouring to fill up gaps, and to do it in a way that will carry with it some inescapable compulsion. Like the scientist also but, as I hope I can show, in a very different manner, in filling a gap, they open up many more.

I have said that everyday thinking differs from the more mathematical and logical, and from the experimental, in that it is directed towards convincing by strong assertion rather than towards compelling by proof. And I called this a strategical difference, because it defines the broad plans within which the particular techniques—the tactics—of thinking must be made to work. It may seem as if artistic thinking shares both these strategies, for it is set to achieve a terminus which every person of sensibility must at least appreciate, even if he will not "accept" it, because of its own character and not because the artist or anybody else asserts that he should. It seeks the universality of the mathematical, closed system and experimental kind of thought, and at the same time the freedom from demonstration and proof of the everyday thinker.

For my part I am unable to accept any view which treats the artist's thinking purely as a combination of the strategies and

tactics of other classes of thinker. I shall attempt to show that artistic thinking has a plan of its own and modes of its own, although this does not mean that it cannot ever share in other designs and modes, or that other designs and modes cannot, on occasion, join with those of the artist. It should be clearly understood that I am considering the thinking of the artist at work, not the thinking of the artist *about* art.

2. STARTING-POINTS

Fundamentally all the kinds of thinking that we have been discussing start from evidence of the sort that everybody calls factual. The evidence consists of items—which may be treated as exceedingly simple or as complex up to the limits of what can be regarded as a "whole"—laid out in a sequence having an internal gap to be filled by interpolation; or stopping short of a terminus and requiring extrapolation; or demanding reshuffling and reinterpretation. The artist, being human, must of course use the same materials as everybody else to think with—the things he can see and hear, his perceptions, and whatever more complex and individual combinations he can make out of them. He cannot, however, begin his artistic thinking until he has built something from them. He cannot just take things as most folks say that "they are."

A daughter of Rudyard Kipling wrote: "During my father's life a large canvas-covered case labelled 'Notions' lay on the desk, containing unfinished stories, notions and ideas collected through the years."[1] These were his materials, the incompletenesses of which from time to time he would fill out, producing the more finished poem or story. She has described the episodic way in which he often worked—pacing about, stopping short, sitting down, intolerant to any form of interruption, and then, again, entirely relaxed.

The manners—or mannerisms—differ from artist to artist, of course, but the materials which he will work with are never taken at the face value of their immediate appearance as the everyday thinker claims that his materials are. They are not

[1] *Rudyard Kipling: His Life and Work*, by Charles Carrington (London: Macmillan and Co., p. 514).

necessarily so taken, either, by the scientist or by the closed-system thinker, but in the preparations of evidence which these effect there remains nothing individual; they are, as shaped and as used, "the same for all."

I recall how one day I was walking in a Turkish city with an artist. "You see those men?" she said. They were men of working class, sitting, perhaps very tired, in a row, on a rough seat, by a wall. "Some day," she said, "I shall paint a picture of those men." I asked her, "Why not now?" "No," she said, "it is not ready yet."

People have used all sorts of words for the preparation without which the artist cannot really begin to think: vision, revelation, insight, dream, illumination, and, of course, with many shades of difference of implication, sensibility. Plenty of people, artists and all, especially those who will rush into generalization, saying such things as that all art is nothing but a peculiarly faithful form of reflection, assert that such preparation is simply to "see things as they really are." This, however, seems to be little, if anything, more than a way of saying that when the artist's preparation is completed there remains something that nobody else can perceive or appreciate unless the artist helps him.

3. STEPS

Whatever form the preparation may take, it reveals to the artist a gap which he must try to fill, in the medium in which he is expert. Each available medium has to be cast into a form of sequence. In some cases, as when, for instance, words or musical tones are used, the eventual sequence achieved remains in the final production. In others, as when, for instance, colours, shapes, masses, and light and shade are pictorially rendered, the sequence has disappeared in the final achievement.

Two different sorts of step series, which for most purposes, including our own, must be carefully distinguished from one another, seem almost always to be required. The one consists of steps in craftsmanship, more apparent, probably, in painting, sculpture, and various forms of acting than elsewhere. The other consists of actual stages, temporary halts, in the progression to

the final production, as when the painter suspends for a while the use of the materials of his craft, steps back and considers the position he has reached, and how he must go on; or when the writer lays down his pen, reads what he has written, and then builds up from this, or starts afresh. It is the second of these step series with which I am particularly concerned, the first being in fact exercises in special bodily skills which lie outside the range of this discussion. The second is the series most nearly parallel to those step sequences used to fill other gaps which were the subject of much comment in the earlier portions of this book; but they have characters of their own.

People who write about art and artists are very fond of pointing out that there is a third step series, as the artist moves from his starting-points to his finished work. This, it is alleged, lies behind the other two. It consists of the decisions which prompt the artist to take brush, or pen, or chisel; of the often-obscure impulses, needs, wishes, and desires whose issues may appear as stages of performance. It is a succession of feelings, deep laid, frequently unacknowledged, and even, by their possessor, unknown, which may shape both the craftsman's technique and the form of the artist's achievement. I do not doubt that there is this third series of steps, or that it is a fit and fascinating topic for psychological speculation. Apart from speculation, however, there are two ways only in which the features of this underlying move sequence can be studied. One is by asking the artist himself, and perhaps helping him to answers by some special technique of question; the other is by inference from a direct consideration of the performance stages.

The former has been tried often, especially during the last thirty years or so. It has produced much that is interesting, more that is highly confusing, but little indeed that can be regarded as assured. The latter, but without the inferences, is what I have attempted in the more or less parallel instances—of the closed-system thinker, of the scientist, and of my own procedure—from other fields; and it is what I shall attempt here.

We found that one of the most characteristic features of all the thinking procedures in the closed system is that, as the number of steps taken towards filling up a gap increases, the number of probable next steps decreases, until a stage in the

sequence is reached beyond which all thinking must proceed through the same number and order of steps to the same terminus. Although experimental scientific investigation, in its early stages, opens up a great many more paths for probable exploration, and so the course of its journeyings is apt to be much more wandering, it has the same character, in the sense that once a halting-place has been scientifically justified, as it is neared the steps to it become more and more the same for everybody. It is true, however, that effective scientific experiment always closes at least one gap, but opens a number of others.

The thinking of the artist does not proceed in this way. Perhaps the most pointed case to take for illustration and analysis is the novelist at work upon a long story. His preparation sufficiently completed, he launches his task. As his characters gain form and individuality, and perhaps as more and more of them come into the scene; as incidents open out and play one upon another, the lines for subsequent development that appear possible increase in number. He reaches a temporary halting-place, closes a chapter, or a section. Now he branches into some new direction. It is something like, on a far lower level, rounding a corner in a maze, only round the corner there are an indeterminate number of potential new directions. As step (or, say, chapter) follows step, there is a strong and growing impression that next steps as they might be taken are, not fewer, but more. Yet in the great stories, every step that is taken is "right" and has about it a character of inevitability, not because nothing else could happen, but because it is fitting that precisely this should happen. If it is the complete novel that is being built there generally comes a stage beyond which, both for the artist who builds and for the reader who, so to speak, takes a spectator's part in the work of the building, every next step carries with it a growing sense of inevitability. In the earlier stages the question was, "What now, among more and more and more things that might happen, will happen?" There is room for surprise. But in the later stages question gives way to statement, "If all goes well this is the sort of thing that will happen," and at last, categorically, "This is what must happen. This alone is satisfying."

At some stage, not very often exactly specified by the writer himself, but usually after his story has got well under way, it very often appears to him that something outside himself has taken charge and is now settling everything that happens. Usually he then says that it is the characters themselves that do this. "In setting out to write a novel, the one thing I know definitely beforehand is the end. As I go on, a few peaks in the story begin to show themselves, like submerged islands rising from the sea. Apart from this I give my people a free hand to work out their own lives."[1] It now seems as if the sequence has achieved direction in so definite and outstanding a way that the direction itself has all the marks of objectivity, and the writer has nothing to do but to see and follow. If the artistry is good enough, and the reader is in tune with it, that is all that he has to do also. And yet, certainly the reader, and perhaps also the writer could, if they desired, raise questions, and more and more questions, and all of these could have varied answers; but when the artist is a master they do not need or want to do this. The sanction, however, which provokes and justifies acceptance, is not one of necessary relations of facts; and it is not one of abstract and absolute inevitability; it is one which rests upon the appropriation and understanding of a standard.

The steps through which the artist passes who works in one medium can never be exactly repeated, or translated, in a different medium. This must not be taken to mean that with varying media artistic thinking itself becomes different. It will express different things, and it appears certain that the key incidents in the sequence through which the steps must pass vary from one medium to another, but the basic strategy and tactic of the thinking remain the same. Anybody who has watched a beautiful picture growing as the painter has made it, will know well that the steps have the same character as those which I have already described in the other case of the writer. There is the preparation, which may or may not be with quick sketches, quickly discarded, or in part only to be retained. Then the picture begins to grow, and as its maker moves along the succession of his adventure, the watcher will see how

[1] *Henry Handel Richardson: A Study,* by Nettie Palmer (London: Angus and Robertson, 1950, p. 149).

opportunity seems to be opening out all the time, and may from stage to stage continually ask himself "What next?" The painter, answering in his own medium, may uncover the secret, like Velasquez, with much groping and trying, with many sketches, with rejections and partial acceptances, moving tentatively, and then at last with certainty towards the final picture. Or he may, like Goya, go swiftly, with hardly any apparent exploration, so that if he makes a sketch it is not to help him to answer some occasional question, but is, to all intents and purposes, a model for the final picture.[1]

It is true also that the fine picture, when it has reached a key phase in the succession of steps, moves with greater sureness, with more uniform direction, and generally with an increased energy towards its terminus. The painter, like the writer, may speak of himself as irresistibly driven, but the necessity remains one of conformation to a standard.

Here it is that the artist's thinking comes, perhaps, nearest to the everyday kind, for in many instances it has a large admixture of accepting and using conventions. These, more often than not, are a social product and spring from an interplay of technique and aims in some group of people who, in all other ways, may be far more diverse one from others than is usual in social grouping which does not have an artistic character. Sometimes these conventions are not socially derived, but are the artist's own, shaped by society only in the sense that the particular artist is in revolt against society. Social or individual, however, the artist's conventions—if he is more than a copyist —still remain in the service of a standard, and so, though like the everyday thinker he may be convention dominated in one way, he cannot just use the convention, but must pass beyond it by steps towards whatever standards it serves.

When we were discussing everyday thinking, we found that it is not uncommon, if some incomplete specific information sets the process going, for the thinker to resort at once to a conventional generalization, then to bring up an illustration, usually a personal one, and at once to use it to fill up the gap. It seemed as though there was no actual sequence of steps in this. Does this same kind of thing happen in the case of the artist? Perhaps

[1] See F. J. Sanderez Carton: *Goya* (Les Editions G. Cres et Cie, Paris, 1930).

nobody can be quite certain, but it seems unlikely. If we should find such thinking without sequence it would most likely be when the artist appears to work "in a flash," and the product itself is brief and short—the small, brilliant drawing, the short story with but a single incident, the musical theme, the momentary poem. Lamartine, for instance, was said to compose verses entirely on the spur of the moment, without even the smallest trace of reflexion. In many, if not all of such cases, however, the product seems as if it must be regarded as a fragment, still incomplete, and waiting to be worked, by some new step sequence, into a more finished form. There is indeed a kind of artist's technique which apparently aims to achieve fragments. "Kipling's stories are sometimes cryptic, sometimes obscure, sometimes allegorical. Quite early in his career he developed a technique of leaving the story half told and so maintaining the suspense."[1] Paul Valéry pondering this "flash" occurrence, as it may happen to the artist, was quite certain that it is a process which passes to an achievement lying beyond the "flash" itself.

"In this process there are two stages. There is that one where the man whose business is writing experiences a kind of flash—for this intellectual life, anything but passive, is really made of fragments: it is in a way composed of elements very brief, yet felt to be very rich in possibilities, which do not illuminate the whole mind, which indicate to the mind, rather, that these are forms completely new which it is sure to be able to possess after a certain amount of work. Sometimes I have observed this moment when a sensation arrives at the mind; it is as a gleam of light, not so much illuminating as dazzling. This arrival calls attention, points rather than illuminates, and in fine is itself an enigma which carried with it the assurance that it can be postponed. You say 'I see, and then tomorrow I shall see more.' There is an activity, a special sensitization; soon you will go into the dark room and the picture will be seen to emerge.

"I do not affirm that this is well described, for it is extremely hard to describe. . . ."[2]

[1] Carrington: op. cit., p. 468.

[2] Quoted from Jacques Hadamard, op. cit., p. 17.

So also, if we remind ourselves of the very famous letter written by Mozart about his own musical inspirations, there is just the same strong implication that though the artist's thinking may move from "flash" to "flash" it must do so through a succession of related steps. Sometimes "thoughts crowd into my mind as easily as you could wish. Whence and how do they come? I do not know, and I have nothing to do with it. Those which please me, I keep in my head and hum them; at least others have told me that I do so. Once I have my theme, another melody comes, linking itself to the first one, in accordance with the needs of the composition as a whole; the counterpoint, the part of each instrument, and all the melodic fragments at last produce the entire work."

More often than any other thinker the artist may achieve the materials of his work by flash and leap, but he must then work them into a sequence which, at first opening up more and more opportunities, at length arrives at its terminus.

4. TERMINUS

"Mathematical demonstrations, being built upon the impregnable foundations of geometry and arithmetic, are the only truths that can sink into the mind of man, void of all uncertainty; and all other discourses participate more or less of truth according as their subjects are more or less capable of mathematical demonstration."[1] It is not surprising that these words should have been spoken by Sir Christopher Wren, for of all the arts, architecture is the one that most nearly allows and even invites, closed-system thinking. I have already suggested, however, that the artist's preparation, and also the sequence of steps through which he moves to his terminus, are both compelling in themselves, and yet they give rein to further opportunity. If this is correct it would seem that the artist's terminus should be able to claim acceptance with conviction, and at the same time scope and freedom for variety of interpretation, and power to start new explorations.

Perhaps this is the same sort of thing that Clive Bell said

[1] Quoted from *Sir Christopher Wren*, by John Summerson (London: Collins, 1953, p. 52).

about Virginia Woolf and Picasso: "Virginia and Picasso belonged to another order of beings; they were of a species distinct from the common; their mental processes were different from ours; they arrived at conclusions by ways to us unknown. Also their conclusions or comments or judgments or flights of fancy or witticisms, or little jokes even, were true or convincing or effective or delightful for reasons that are not the reasons of logic nor yet of our well-tried common sense. Their standards, too, were of their own creation; yet spontaneously we appraised by those standards, which for the moment we not only accepted but appropriated, whatever they chose to offer. Their conclusions were as satisfying as the conclusions of mathematics though reached by quite other roads. . . ."[1]

The apparent paradox of artistic thinking, present from preparation to terminus, is that it is at once convincing and satisfying, and yet question-making and disturbing. When the terminus is reached and expressed so that it becomes available to the spectator and the listener these, if they find it the work of a master, generally want to go back to it again and again. But this is not because they have failed to understand it, but because by going back to it they can understand better and more, asking new questions and discovering the answers.

We could say now that there is thinking which uncovers laws, of finished structure or of relations among facts of observation and experiment; there is other thinking which follows conventions of society or of the single person, and there is other thinking still which seeks and expresses standards. The third kind is the thinking of the artist. The law and the convention are outside the thinker. He cannot alter or affect the first, and if he is the everyday thinker, it is an infinitesimal and unobservable amount that he can contribute to the second. The standard remains in part within the thinker and in his affinity with other human beings.

It is largely because of this that the artist's thinking far more frequently proceeds to its terminus by a leap than does thinking in the closed system or in experimental science. In both of the latter cases the direction of evidence must be in some ways picked up from the properties of external data. In the former

[1] *Old Friends: Personal Recollections* (London: Chatto and Windus, 1956, p. 95).

case the direction is inherent in the search, and partly, at the least, internal to the searcher himself.

Once the artist achieves his terminus his work for the time being is done. If he goes on and tries something of the nature of exposition it is as critic, not as artist. His product is not made in the interests of "immediate communication," and therefore he has no need for any kind of violent assertiveness. No doubt there have been artists who have adopted techniques that amounted to sweeping assertion, and others who have set out to communicate some "message," but they have been using art in the interests of everyday thinking. "Having made a beautiful drawing, which might or might not suggest some crude bit of social or political criticism, Daumier as often as not could think of no legend to put under it. The drawing, you see, was not an illustration of something else, but a work of art complete in itself."[1]

The broad strategy of the artist's thinking is neither to compel by proof nor to convince by assertiveness; but to satisfy by attainment, it may be in one medium or it may be n another, the requirements of a standard. In the attainment that is reached there remains something personal to the artist, but also there is something common to all who will accept or can appropriate the standard. The artist's tactics are to prepare the materials which he finds through his observation, not accepting them as other thinkers may do, but working them into the form most fit for his medium and his ultimate design; and then, perhaps with sudden leaps here and there, to make a succession of steps to a terminus which is the artist's particular expression of his standard in terms of the materials he has chosen to use. The step sequence which he effects is never one which converges steadily to a single issue. In its earlier stages every step opens up more diverging paths. Later, characteristically, it reaches some critical stage beyond which each successive step achieves a partial issue which presents itself as the one that is most satisfying, but never as the only possible one.[2] When the artist decides to make his final halt, the

[1] Clive Bell: op. cit., p. 55.

[2] I have often wondered whether an artist, himself skilled in the craft of one of those forms of art in which the succession followed in achievement disappears in

terminus will appear inevitable to all those who accept and understand the artist's standard; but it will not without search display an answer to all the questions that can still be legitimately raised. Thus, it is of all the kinds of thinking that we have considered, the most free from outer compulsion; alike in the variety of the directions which it can explore on its way to a terminus, and in the range and manner of exploration which a terminus reached still leaves open.

5. SOME FINAL REMARKS

I must now return to the starting-point of this study, to the suggestion that thinking could be regarded as a high-level form of skilled behaviour, requiring signs and symbols for its expression, yet still possessing many of the characteristics of the earlier established bodily skills from which it may have developed and which it has supplemented. I then picked out certain properties of the skilled response which seemed likely to be particularly worth looking for in one kind of thinking and another. These were: that all skilled behaviour is set into a form of significant sequence within which it must be studied if understanding is to be reached: that it submits to a control which lies outside itself and is appreciated, at the bodily level, by the receptor system; that proper timing, the ways in which transition is made from one direction of move to another, "point of no return," and the character of direction and how it is appreciated are all critical features of skilled behaviour. From time to time, and in relation to all the kinds of thinking which I have discussed, I have returned particularly to those properties of skill, and it has seemed not only that thinking of all kinds possesses them, but also that their study does throw some real light upon the thinking processes themselves.

At the same time it has over and over again become clear, as the study has proceeded, that each kind of thinking possesses its own balance of these properties, its own characteristic variety

a terminus, all the "steps" of which must be presented simultaneously, could from the simultaneous whole reconstruct the successive moves of production. Could a painter do this for another painter's work, whom he did not in any way know? In particular could he successfully detect the critical phase, the key point, of the succession? I do not know.

of most of them, and some features that are all of its own. When we say, as we now—I should claim—certainly can truly say, that thinking is an advanced form of skilled behaviour, what we mean is that it has grown out of earlier established forms of flexible adaptation to the environment and that the characteristics which it possesses and the conditions to which it submits can best be studied as they are related to those of its own earlier forms. But they are not the same as those of the earlier forms, and they are not necessarily related one to another in either the same, or in some single, order of importance.

One of the gains of the line of approach adopted, I should claim, is that it points the way to a number of more exactly defined methods of experiment on thinking processes. Some of these I have described and used, but the exploration is not final, and it is not anywhere near complete.

I have distinguished four kinds of thinking. They express overlapping, but in some ways differing, strategies and tactics. Here, too, however, the survey is in all probability incomplete. Religious, and especially mystical, thinking may be found to have characters which the other kinds do not possess, as well as to have some of the features of closed system, experimental, everyday, and artistic thinking. It seems to me that very likely an empirical, or experimental, study of legal thinking would show that this also is in various ways an independent kind.

Everybody is almost sure to notice that in the whole of this treatment nothing of any importance is said about underlying motivations of the kind that the thinker himself, and nobody else, can detect, or about the inevitable affective and emotional accompaniments of any adventure into thinking. It is not to be assumed that these are, in my view, of little or no significance. I have left them out, because I consider that we must get straight about the facts and relations exhibited by and in thinking activities in those ways that anybody can discover, before we can be reasonably sure about interpretations of the more introspective sort of evidence.

Although thinking is in various ways tied to a prolonged past history and to the varied skills of the body, it has also achieved emancipation from them. It is with thinking as it is with recall: "Memory and all the life of images and words which goes with

it, are one with the age-old acquisition of the distance senses and with that development of constructive imagination and constructive thought wherein at length we find the most complete release from the narrowness of presented time and place." The skills of the body are attached to the demands of the world of the moment, but thinking can meet those far away in any direction and even those which have no time or place. And because, now, body movement is supplemented by signs and symbols—all the varied media of its expression which thinking can employ—the thinker is in a position to attain a vast increase in the range and delicacy of his adaptability.

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